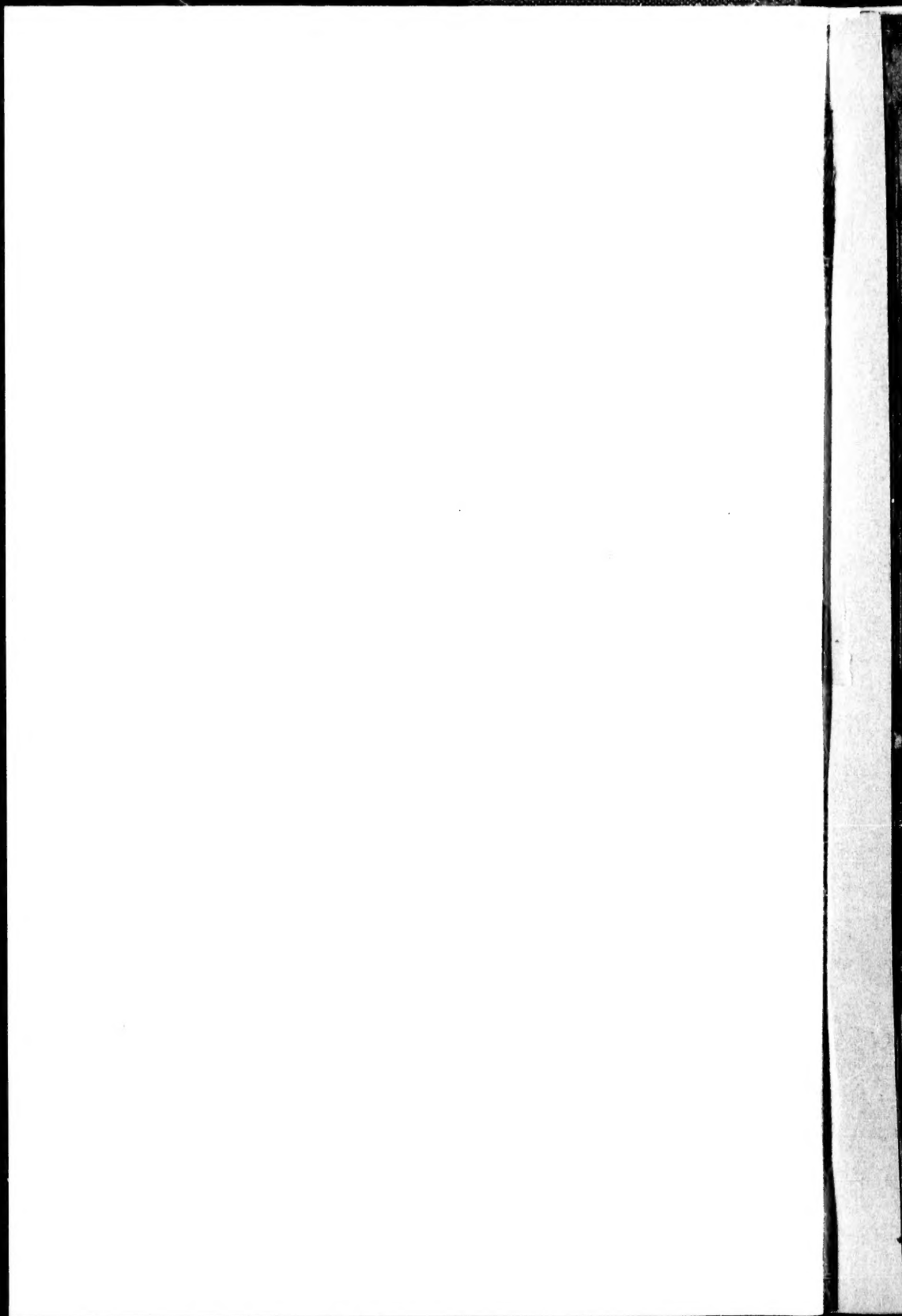




H. Canning

REPORT AND CRITIQUES

OF
E. S. DE ROTTERMUND, Esq.,

LATE CHEMICAL ASSISTANT

TO THE GEOLOGICAL SURVEY OF CANADA IN 1884

MONTREAL:
PRINTED BY JOHN LOYD, 45, RUE DU PARLAMENT.



John D. MacKenzie,

Ithaca, N. Y.,

April 10th 1912

from Mr. Keble.

REPORT AND CRITIQUES

OF

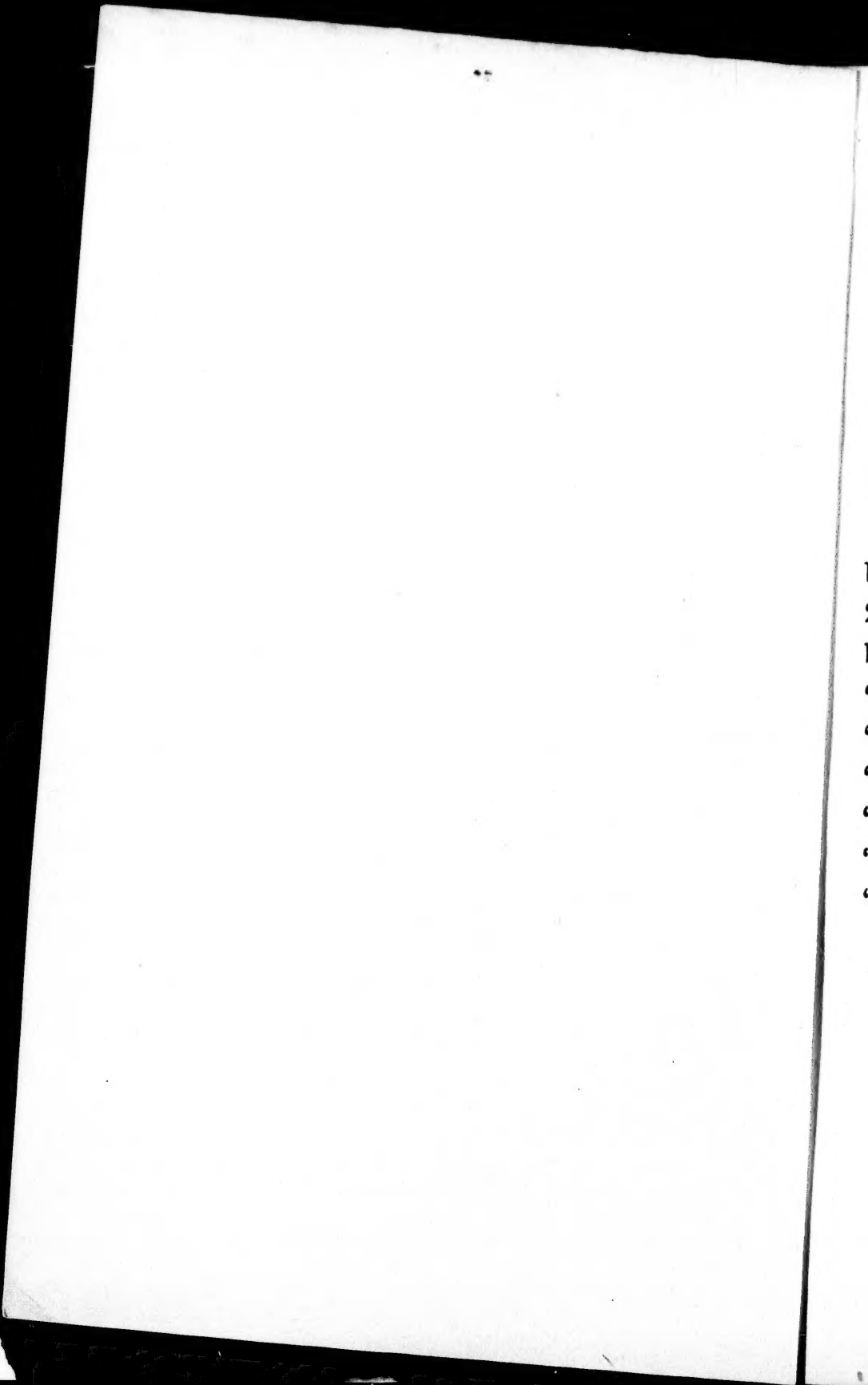
E. S. DE ROTTERMUND, Esq.,

LATE CHEMICAL ASSISTANT

TO THE GEOLOGICAL SURVEY OF CANADA, IN 1846.

MONTREAL:

PRINTED BY JOHN LOVELL, ST. NICHOLAS STREET.
1850.



REPORT.

RETURN to an Address from the Legislative Assembly to His Excellency the Governor General, dated the 23d instant, praying that His Excellency would cause to be laid before them, "Copies of any Report or Reports from E. S. de Rottermund, heretofore Chemist to the Provincial Geological Department, or to the Provincial Government ; and, also, of all correspondence between the said E. S. de Rottermund and the said Department, or between him and the Provincial Government."

By command,

D. DALY,
Secretary.

Provincial Secretary's Office,
Montreal, 29th May, 1846.

- No. 1. Letter from the Provincial Geologist to Mr. Assistant Secretary Hopkirk, dated 26th March, 1846.
- No. 2. Letter from the Provincial Secretary to the Inspector General, and to the Receiver General, dated 27th March, 1846.
- No. 3. Letter from Mr. Assistant Secretary Hopkirk to the Provincial Geologist, dated 16th April, 1846.
- No. 4. Letter from the Provincial Geologist to Mr. Assistant Secretary Hopkirk, dated 17th April, 1846.
- No. 5. Letter from Mr. E. S. de Rottermund to the Provincial Secretary, transmitting his Report as Chemical Assistant to the Geological Survey of the Province, dated 17th April, 1846.
- No. 6. Letter from the Provincial Secretary to the Provincial Geologist, dated 22d April, 1846.
- No. 7. Letter from the Provincial Geologist to the Provincial Secretary, dated 23d April, 1846.
- No. 8. Letter from Mr. E. S. de Rottermund to the Provincial Secretary, dated 23d April, 1846.
- No. 9. Statement of the Provincial Geologist in reference to the late Chemical Assistant to the Geological Survey of the Province.

No. 1.—*Letter from the Provincial Geologist to Mr. Assistant Secretary Hopkirk.*

MONTREAL, 26th March, 1846.

SIR,—Mr. De Rottermund, the Chemical Assistant to the Geological Survey of the Province, having some time since intimated to me verbally his intention of relinquishing his connection with the Government and having ever since ceased attendance in his laboratory, I am not exactly aware how much of his salary may be due him. It will therefore be unnecessary to make out a Warrant in his favour until further notice.

A Warrant for my own quarter's salary, and that of my Geological Assistant, Mr. A. Murray, can be made out as usual.

I have the honor to be, Sir,

Your most obedient servant,

W. E. LOGAN,
Provincial Geologist.

James Hopkirk, Esquire,
Secretary's Office.

No. 2.—*Letter from the Provincial Secretary to the Inspector General, and to the Receiver General.*

SECRETARY'S OFFICE,
MONTREAL, 27th March, 1846.

SIR,—I have the honor, by command of the Administrator of the Government, to acquaint you that an intimation has been received from W. E. Logan, Esquire, Provincial Geologist, that E. S. de Rottermund, the Chemical Assistant in his Department, has for some time past ceased to attend to his duties, and requesting that, under these circumstances, His Excellency would be pleased to direct that no Warrant should be passed for his salary to the 31st, until further notice.

His Excellency has therefore been pleased to direct that his Warrant should not be passed or paid until further notice.

I have the honor to be,
&c. &c. &c.

(Signed,) D. DALY.

The Inspector General.

Similar Letter to The Receiver General.

No. 3.—*Letter from Mr. Assistant Secretary Hopkirk to the Provincial Geologist.*

SECRETARY'S OFFICE,
MONTREAL, 16th April, 1846.

SIR,—With reference to your letter of the 2nd instant, reporting the resignation by Mr. de Rottermund, of his situation as Chemical Assistant to the Geological Survey of the Province, I have the honor, by command of the Administrator of the Government, to request that you will acquaint me, for His Excellency's information, at what period Mr. de Rottermund's connection with the Geological Survey ceased, and up to what period therefore his salary should be paid to him.

I have the honor to be, &c.

(Signed,) JAMES HOPKIRK.

W. E. Logan, Esquire,
Provincial Geologist.

No. 4.—*Letter from the Provincial Geologist to Mr. Assistant Secretary Hopkirk.*

MONTREAL, 17th April, 1846.

SIR,—I have the honor to acknowledge the receipt of your communication of the 16th instant, requesting information in reference to the amount of salary due to Mr. de Rottermund, for services rendered as Chemical Assistant to the Geological Survey.

In reply I have acquaint you, for the information of His Excellency the Administrator of the Government, that in the correspondence I have had with Mr. de Rottermund on the subject, he informed me that he claimed no part of his salary for the last quarter, which in reply to him I have qualified by saying I understand to mean no part beyond such an amount as might be considered justly due in proportion to the time which he had devoted to the duties of his Office. But Mr. de Rottermund has since afforded me no opportunity of ascertaining what the amount may be.

I have further to state, that on the 20th December last, about four weeks after my return from an exploration of the Ottawa,

I, in writing, requested Mr. de Rottermund to furnish me with a simple list of the substances he had analysed and their constituents ; that I have since several times verbally, and twice in writing, requested the same list, which would be merely a copy of the register which no doubt has been kept in his Office and might be made out in a few hours. But though this is necessary for the purpose of enabling me to judge of the amount of work that has been performed in the Chemical Department of the Survey, it has not been furnished.

I have the honor to be, Sir,

Your most obedient servant,

W. E. LOGAN,
Provincial Geologist.

James Hopkirk, Esquire,
Secretary's Office.

No. 5.—*Letter from Mr. E. S. de Rottermund to the Provincial Secretary, transmitting his Report as Chemical Assistant to the Geological Survey of the Province.*

(Translation.)

MONTREAL, 17th April, 1846.

SIR,—I have the honor to present you my Report on the labors confided to me. If I have not been able to make it as complete as I could have wished, it is because various circumstances have prevented me from doing so, and at the same time placed me in the necessity of tendering my resignation. I refrain for the present from stating the motives which induced me to suspend my labors. I am far from refusing my services to contribute to the development of this branch of the work ; but at the same time I take the liberty of stating that I will be happy to continue, if the Government will allow me a certain latitude which I find necessary.

1st. That I may receive orders from none but yourself.

2ndly. That I may have an Assistant in my Laboratory, as it is utterly impossible for me to work alone.

3rdly. That I may, for the future, receive no orders from Mr. Logan.

My Report will prove to you that the labors of chemists must not be impeded, and are of the greatest advantage to the

country. The chemist requires a certain latitude in his researches which are known to himself alone. In order that he may be really useful, he must not be treated as a clerk,—in which light he has been considered by Mr. Logan in his letter of December. I have had but thirty pounds for my scientific tour; and offers have already been made to the Government of several thousand pounds, which are solely to be attributed to my chemical labours. I do not ask for any increase of salary, but I beg the Government to remark that my demands are not unjust. The Chemical Survey costs only £400, and the Geological Survey £700; it is not too much then, to ask for £100 to facilitate researches which will be of so great advantage to the country. If it please the Government to order me to make an exploration, let the sum required be determined by the Government; for it is the interest of the country that scientific researches should not be controlled by personal feeling. Besides, the Executive Council will know before every one else where the riches of the Country are, and their importance, which is not the case in the present state of things:—there is now no control,—no certainty whatever. The Chemist and the Geologist will then have each their own Department without encroaching upon the rights of each other, and each one will claim the honor due to himself as a man of science.

I have the honor to be,

Your most obedient servant,

E. S. DE ROTTERMUND.

Hon. D. Daly,
Provincial Secretary,
&c. &c. &c.

MR. E. S. DE ROTTERMUND'S REPORT.

After having analysed the waters of the St. Lawrence and of the Ottawa opposite the city of Montreal, I again proceeded up the river Ottawa as far as the "Graisie" River, and from thence continued to the township of Hawkesbury to Caledonia and George's Lake. I came down to Lancaster, and then went

up the St. Lawrence again as far as Kingston. After visiting the mineral springs on Prince Edward's Island, I went in the direction of Toronto by Belleville and the River Trent. I visited Lake Simcoe as far as the township of Georgiana. I went afterwards to Hamilton and examined its environs. I continued as far as Brantford and Woodstock, and then returned

Hamilton by St. Catherines and took the direction of Niagara, and finally returned to Montreal by the St. Lawrence. The mineral springs which exist in the different localities I have just mentioned are divided into magnesian, ioduretted, saline, antimony-ferruginous, sulphureous, and gaseous. The magnesian and ioduretted springs are principally at Caledonia, on George's Lake, and the "Graisie" River. Those which I visited in the neighbourhood of Lake Simcoe and at Woodstock, are magnesian. The saline springs are at Kingston and Prince Edward's Island. Near Hamilton and at St. Catherines the sulphureous springs are very weak, and so are the gas springs, and are always accompanied by magnesia. In fine, the antimony-ferruginous springs are at Brantford. The name of "ioduretted springs" does not mean that the iodine predominates over all the other substances held in solution, but that this substance being very scarce and of great medical value, I have given the waters which contain it, that denomination which is used by several men of science. For the same reason I have called the acid springs of Brantford antimony-ferruginous, on account of the presence of that substance, only recently found in mineral waters, and heretofore unknown in their composition; and because it is one of the most remarkable substances as a remedy for several diseases. There are mineral springs called "acid springs," from the presence of carbonic acid, such as that of Vichy in France, where whitelead is manufactured. In order that the above mentioned spring be not taken to be similar to these, I have given it the name proper to its composition.

Having described the nature of the various springs and their respective positions, I will now give a description of each one in particular. On the River "Graisie," in the township of Hawkesbury, Ottawa District, there are several mineral springs

very close to each other. The first is on the left bank, at a distance of three miles from Mr. Jamieson's residence, and two miles from Mr. M'Nab's mill, on Mr. M'Kinnon's land, lot No. 3, of the sixth concession, and is situate at the foot of a tree a few paces distant from the river. This spring disengages in a very small quantity hydro-sulphuric and carbonic acid gases. The water is magnesian, and contains chloride of magnesium and sodium, sulphate of magnesia, and carbonate of lime. It deposits a very small quantity of sulphur on the surface of wooden vessels, which is owing to the decomposition of the hydro-sulphuric acid. This spring is very close to the river, and is covered by the high waters. Its temperature was 46° , that of the atmosphere being 74° (Réamur). The soil is blue clay.

At a distance of a quarter of a mile from the first spring there are four others, which are almost in the river; one of them disengages carburetted hydrogen gas, and carbonic acid. These springs contain neither lime nor sulphuric acid combined, but chlorides and carbonates. Their temperature was 44° , that of the atmosphere 33° (Réamur).

At a distance of fifty or sixty paces from the preceding ones, but further from the river, and in the depth of the wood there is another spring which, with a slight exception, is similar to those already mentioned. It contains some traces of iodine and more salts in solution, which is probably owing to its being at a greater distance from the river, and consequently less affected by the high waters. Its temperature is the same, and the gases it evolves, of the same nature. The soil is also similar clay, but not quite so blue. But these springs cannot be brought into use, on account of their being covered by the waters of the river in the spring and autumn.

THE CALEDONIA SPRINGS.

The mineral springs known under the name of Caledonia are three in number. The first, near the hotel, is a gas spring; its specific gravity is 1.0038. The second is saline: its specific gravity is 1.0087. The third is sulphureous: its specific gravity is 1.0062. The temperature of all three is 44° , and

they are situated in a ravine at a few paces distance from each other. The first disengages a great quantity of carburetted-hydrogen gas mixed with carbonic acid and oxide of carbon. The second disengages the same gases, but in a very small quantity. The third hydro-sulphuric acid also in a small quantity. These gases are disengaged in much greater quantity before a storm; they diminish with the heat, and are in still less quantity in very hot weather. I think this is owing to the atmospheric pressure which may condense the gas by its action on the surface of the water, while another cause may exist with relation to the springs which disengage hydro-sulphuric acid. It may be supposed that it arises from the sulphuret of iron or pyrites with which the country abounds, and which is found in the different beds of earth, being dissolved in much greater quantity by the water in the seasons in which they are most abundant, and in which the decomposition of the sulphuret is more facilitated than at a period when the spring is fed from one part of the bed only.

An analysis of the Caledonia waters had already been made by Mr. Chilton of New York. I am sorry to remark that they do not agree with his analysis, not as regards the quantity of matter, but principally as regards their composition and the existence of substances in them. According to him, these waters contain sulphate of lime, iron, and potassium. I have not even been able to detect any traces of these substances; consequently I think that Mr. Chilton has taken carbonate for sulphate, and has found the iron and potassium in the ashes which may have been blown by the wind into the springs, or in the bottles which contained the water, if they had not been well cleansed.

As magnesian, ioduretted, and sulphureous springs, they are very useful, and on account of the temperature which is always regular, are excellent for rheumatism and gastric diseases; they are besides useful for diseases in the nature of *goitre*, for which iodine is so efficacious a remedy. The soil is reddish clay; underneath, at a depth of one foot, the soil is blue clay of the same nature as that of Hawkesbury. The Caledonia springs are in a plain, surrounded by marshes. The

atmosphere, however, is always healthy, as it contains nothing of that heaviness generally produced by marshes. The soil is good for the purposes of agriculture, for, though argillaceous, it contains a very thick layer of vegetable earth; but the country people lose all its advantages by burning it, as by doing so they leave nothing but the clay. The vegetation is very rich before the fire has passed over it, which proves that agriculture would succeed very well in this country if the manner of clearing were different. The soil might easily be improved by lime which abounds at a distance of a few miles. Near Caledonia, in the direction of Vankleek Hill, there is a marsh twelve miles in circumference, which contains turf of the thickness of a foot. It might be made use of with great advantage as regards economy, especially when wood becomes scarce.

Besides the three springs above mentioned, there is another in the wood at a distance of two miles from the settlement. Professor Williamson, of Kingston, analysed a spring at this place, which he styles intermittent. It must have been of great value; for according to the analysis, it contained 1.7 gr. of bromine and 0.3 iodine. Unfortunately the spring answering to the description has disappeared. Two paces from its site another has formed itself, of a totally different nature. It is entirely of a gaseous nature. Its gas is carburetted hydrogen, and no traces can be discovered of the hydro-sulphuric gas which the former spring contained. This new spring disengages the gas in great abundance, and not at intervals. It is situate at a distance of about thirty paces from the marsh, near a ravine which is filled with water. The soil is blue clay, a little reddish at the surface. This spring is separated from the Caledonia springs by a marsh two miles in width, and which has no solid bottom, unless at a very great depth. This marsh is full of moss and plants in a state of decomposition, on which various trees grow, of the fir species, and the Labrador tea-plant. I think that formerly a lake must have existed, the waters of which have partly run off, and the surface has been covered with vegetation of different sorts. This sponge-like substance formed of moss and marsh plants imbibes the water, which (with the plants in a state of decomposition,) serves as

a nourishment for the trees, which form a complete forest. If report be credited, several mineral springs are to be found there. But on account of the great difficulty of making a passage through the wood and marsh, I resolved not to make expensive researches which might perhaps turn out of no use.

GEORGE'S SPRING.

From Caledonia I proceeded to George's Spring by land, so as to judge better of the changes in the soil. Three miles from Caledonia there is a marsh which extends north and south, and through which the road is cut. Before reaching the springs you descend three rocky ledges towards the north, and you then come to George's Lake, to the south of which there is a mountain. At the foot of this mountain is a mineral spring which flows into the lake. Its components are the same as those of the Caledonia springs, besides a certain quantity of sulphate, and more iodine. This spring flows from under a rock of calcareous shale which contains a very great quantity of iron pyrites; the water contains no traces of iron nor of hydro-sulphuric gas, which would lead one to suppose that it proceeds from another bed. As the proprietor of this spring does not reside on the spot, and the house which contains it being shut, I could not examine nor ascertain the nature and quantity of the gas which disengages itself, nor could I find out the correct temperature. The rust which deposits itself at the mouth of the spring is in consequence of the water discharging itself through an iron tube which is covered with rust. The water which I analysed, though it had passed through this pipe, contained no traces of iron. This spring is situated at a distance of from twelve to fourteen miles from the Caledonia Springs. It is at least two hundred feet lower, and flows to the north; those of Caledonia flow to the south.

Proceeding from Caledonia to Alexandria, I passed by Vankleek Hill, where the streams are very clear and contain carbonate of lime in solution. Limestone which contains fossils is found as far as Alexandria, and the soil is covered with hard wood of different species.

KINGSTON.

The Kingston mineral springs are of a saline nature, and contain salts of lime and magnesia. The spring belonging to Mr. Morton has been analysed by Mr. Williamson; he found hydro-sulphuric acid, but I could detect no trace of it, either by the salts of silver or by the salts of lead. Not that I doubt the correctness of Mr. Williamson's analysis; but on seeing this difference, I could not understand how the nature of this spring could have changed; and after a more attentive examination I found out the cause. Mr. Morton caused the spring to be dug to the depth of one hundred and forty feet. By this means the orifice became much wider, and allowed the water flowing from the different beds to fill the well. This water being of a different nature from that of the spring, by mixing with the latter has caused the change. He had also inserted a leaden pipe and an iron pump; it is natural that if the spring contained hydro-sulphuric and carbonic acid, the lead and iron would be attacked and form sulphret and insoluble carbonate of lead. It is difficult to form exact conclusions as to the gas, for, by the motion of the pump, it is separated from the water, the temperature of which is increased by the same cause. But in order to be certain that this supposition is correct, it would be necessary to know the temperature of the water in the spring; when it comes out of the pump its temperature is 49°

PRINCE EDWARD'S ISLAND.

At Bloomfield the spring is situated in a garden at a distance of a hundred feet from Mr. Stewart Christie's house, on the eleventh lot of the second concession. The soil is blue marl, under which lies fossil limestone. The water of this spring is very saline, and may be used with advantage for the manufacture of salt. It contains lime in solution, which diminishes its value in a commercial point of view. Its depth is forty-five feet, and its specific gravity 1.00721. The level of the water in the driest season is five feet below the soil. This spring is in very bad order, filled with pieces of rotten wood and leaves of trees. It was difficult for me to ascertain the nature of the

gases which it contains. It is certain, however, that it is not hydro-sulphuric acid. This spring might easily, without expense, be so improved as to serve for the manufacture of salt, inasmuch as there is another of the same nature at a distance of twenty feet further. At Picton there is a spring on the southern declivity of the mountain; the soil is the same as at Bloomfield, but it only contains water in the spring and autumn. There are several mineral springs on Prince Edward's Island, but they dry up in the very hot weather. If care were taken to prevent evaporation by the rays of the sun, they might possibly be made use of with advantage throughout the whole year.

Near Belleville, at a few acres distance from the landing place, there are several gas springs in the river. In two places the ebullition is more than two feet in diameter. This gas can be no other than marsh-gas or carburetted-hydrogen, caused by the decomposition of organic substances. At a short distance there is a marsh covered with water; the presence of fish in this marsh is a proof of the nature of the gas, for if it contained any signs of hydro-sulphuric acid gas, no fish could exist in it.

Four miles from Belleville on the road to Trent, there is a saline spring at the foot of a tree on Mr. Leman's property, on the border of Prince Edward's Island, Victoria District. The soil is calcareous. Not far from this spring, on G. Caly's property, there is lead ore. I visited several mineral springs at Trent, among others one on Mr. Ford's property, on the broken front of lots 7 and 8, in the township of Murray, two miles from Belleville. The inhabitants of the locality consider it as the most important one in the vicinity; I found it to contain magnesia; the gases are disengaged in very small quantity. Mr. G. C. Bull, a merchant of the place, assured me that in spring and in autumn they are disengaged in much greater quantities. In my opinion these springs are of no importance, as their strength is not always the same.

TORONTO.

There is also a mineral spring here, on Lake Ontario, almost opposite the Wellington Hotel. It is magnesian, but too weak to be of much use as a medicine. It is more worthy of

notice by its low temperature of 42° (that of the atmosphere being 76°) than by the quantity of salts in solution. This spring is of no use from the manner in which it is kept.

LAKE SIMCOE.

In the township of Georgiana, near Lake Simcoe, there is a mineral spring known as Jefferson's spring. It is near Thorah, on lot No. 10, in the third concession, in a small ravine which intersects the Thorah road. There are two springs there very close to each other; one of them gives very good drinkable water, the other, notwithstanding the quantity of water it affords in a season so dry at this period, contains enough magnesia to give it a taste; there are hardly any traces of the presence of hydro-sulphuric gas. The proprietor states that at other seasons this spring disengages a very remarkable quantity of that gas. The soil is the same as that of Trent. It is therefore possible that the iron pyrites or other sulphurets contained in a certain portion of the layers of earth, are decomposed by the organic matter, and the hydro-sulphuric gas disengages itself. Ten miles from this place the inhabitants of the locality have discovered three other springs in the woods, also in the direction of the ravine; but these springs disappear with the clearing of the forests.

LAKE ONTARIO.

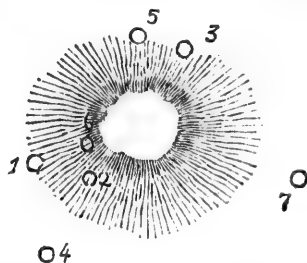
Between Wellington Square and Toronto, in the fourth concession of Dundas Street, Township of Nelson, on Henry Sovering's farm, two arpents from the road and three from Lake Ontario, there are two saline springs close to each other, and a few arpents further there are two others. These springs are in a soil of red shale, and contain a very considerable quantity of chloride of sodium or muriate of soda, without lime, in which they differ from those of Kingston and Bloomfield. The depth of these saline springs is fifty two feet; it is a great pity they are not made use of. The specific gravity of the water at the surface, is 1.0159.

Four miles from Brantford, on the Grand River, in the third concession, lot 26, of the township of Tuscarora, there are springs known as "Sour Springs." I think it would be neces-

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sary to change that name for one more characteristic, according to their composition and nature. I will therefore call them antimony-ferruginous. These springs are in the centre of a splendid forest of oak, beech, fir and maple trees, at a few miles distance from the river. They are rather on a table land of the mountain, than in a plain. There are four springs constantly filled with water, even in the driest season, and seven in the three other seasons. They are all acid and disengage hydro-sulphuric and carbonic gas, bubbling up with great violence. These springs contain sulphate of protoxide of iron, sulphate of alumina and of potash, chloride of antimony, chloride of zinc, sulphate of magnesia and of lime, resinous substances, and vegetable albumen. The temperature of the water is 47° . These three springs are at the base of a small hillock seven feet in height, which was once covered with trees, which have since been cut down, leaving only the stumps. The earth is filled with sulphur crystallized in very fine grains. It appears to be in the state of decomposition of organic substances. The soil is clay, covered with several inches of vegetable earth. It is so acid that it changes blue paper into dark red, and finishes by destroying it as strong acids do. What is more remarkable is, that the earth which has this property is taken from the summit of the hillock. Notwithstanding the composition of all these springs is the same, they must be of different strengths as regards the quantity of salts in each—for the effect of each is different, and so is the taste. Their relative situation is shown by the following figure :



The numbers are in the order of the quantity of water which each spring furnishes. No. 1 furnishes more than twenty gallons per minute. The inhabitants make use of these waters

in different diseases; Nos. 1 and 2, internally; No. 3, for inflammation of the eyes; No. 4, to wash scorbutic and venereal sores. No. 2, is considered very efficacious for gonorrhœas. As Nos. 5, 6 and 7 do not exist at all seasons, they are not made use of. I have taken these waters in tolerably large quantities. I have found that they act chiefly on the nervous system and the circulation of the blood, and cause at the same time diarrhœa. I did not at all feel that heaviness and fatigue generally felt after drinking other mineral waters. I know that several diseases have been cured by means of these springs. A woman of Brantford had a disease of the skin, and the epidermis was so attacked and the blood so corrupt, that the flesh under her nails was affected. By drinking No. 1, and washing with No. 4, she was cured, as several persons of the locality affirm. A mechanic who had wounded his hand with a piece of glass, more than a year before, was completely cured by washing with No. 4. Another cured a gonorrhœa by drinking No. 1. A third a venereal sore by washing with No. 4; and there are a great many other examples of various cures. It is to be remarked that the strength and quantity of these waters increase with the moon. It is my duty to call attention to the importance of these springs, so that Government, in the interest of humanity, may encourage any person who would provide convenient means for using them, so as to obtain the benefits and advantages which might result therefrom in a medical point of view. It may be useful to remark that if water containing zinc and potash, as well as alum combined with potash, is very scarce, water containing antimony is still more important on account of its still greater scarcity and medical power. The salts of antimony are of the greatest value, on account of the great difficulty of preparing them properly; for the chloride of antimony becomes decomposed in water, while here it is produced by the presence of the acid and of organic matter. The discovery of antimony in a mineral spring is undoubtedly an immense advantage for the science of medicine; for the same substance prepared artificially is never so efficacious as when found in water, and no one has succeeded in imitating mineral water, such as those

of Carlsbad, Baden, Aix-la-Chapelle, &c., &c., &c. No imitation could have the same medicinal virtue. I will take the liberty of explaining the theory of the formation of this anti-moni-ferruginous spring, and of explaining the cause why the antimony is in solution without being decomposed or precipitated by the water. According to geological researches it is known that this place contains a great many marshes, iron pyrites, lead ore, zinc and antimony, as well as beds of gypsum. Water running through iron pyrites or gypsum, by some chemical or other phenomenon, becomes decomposed and charged with a quantity of sulphuric acid; (the waters of this spring prove its presence in a very decided manner;) the water so acidulated, passing through turf or marsh covered with vegetation, must contain vegetable albumen, (which I have found,) organic acids, resinous substances, &c. If the water thus charged passes afterwards through beds of ore of antimony, it is natural that it will dissolve the substance without precipitating it.

I will not enter into further details respecting this spring, for as I consider it of the greatest importance in medicine, as well as to the scientific world, I will communicate to you a memorandum when I shall have been able to procure the several instruments which are most necessary for making the complete analysis of these waters, and which I have not in my laboratory at present. I think it would not be useless for me to communicate here my observations on this place, with respect to its commercial and industrial importance. The geographical position of Tuscarora, with its fine navigable river uniting Brantford with Lake Erie, is most advantageous. Great benefits would arise if it were occupied by inhabitants having a legal right of possession, and not by persons who seek only to avail themselves of the possession they have obtained without right. For this reason, the Indians ought to be allowed to make legal sales. Several settlers have already established themselves there under the simple protection of the forest, without having any right to acquire lands. Without improving the cultivation of the land, they destroy the forests which are of the greatest value, especially for this country, where wood is

required for building and for navigation. If they continue, these forests will disappear without being of any advantage to any one. The soil as well as the climate of this township is of the highest value for agriculture. I think that if Government allowed the Indians to sell the land, it would be of great use if it were in the hands of able and educated persons who would know how to turn it to as much advantage as possible, and thereby contribute to the commercial and industrial interests of the neighbouring towns, such as Hamilton, Brantford and London.

WOODSTOCK.

The mineral waters near Woodstock are magnesian, and disengage hydro-sulphuric acid. These springs are of little value, being flooded by streams. If the course of these streams were turned, the springs might possibly be of some use in medicine, especially for the inhabitants of the locality. There is a spring in the Brock District, near Mr. Riddell's lands. As it was dried up, I was not able to ascertain its nature.

ANCASTER.

On the way from Brantford to Hamilton, in the Village of Ancaster, there is a road on the left at the corner of the hotel, which leads to a magnesian sulphureous mineral spring. It is in a deep ravine on the left of the road going up the stream, on Mr. Griffin's property. The nature of this spring is the same as those in the townships on Lake Simcoe.

The spring, known as the "Burning Spring," is in the township of Salt Fleet, 4th concession, six miles from Hamilton and three from the road leading from Hamilton to St. Catharines. This spring is on the same stream as that of Ancaster, a hundred feet above that spot. Its gas is carburetted hydrogen, and not hydro-sulphuric. This spring contains a great quantity of lime; as there is also carbonic acid in this spring, it would be useful in the manufacture of white lead (*ceruse*).

On the left of the road leading from Hamilton to St. Catharines, there are saline springs which are used in the manufacture of salt. One of these springs is already aban-

done, but that of Messrs. Dougall and Kent is in full work ; its depth is three hundred and seventy-five feet. It proceeds from red shale, such as that of Wellington Square. This spring is near a ravine at a small distance from Lake Ontario, in Salt Fleet, 1st concession.

ICE SPRING.

The famous spring known as the "Ice Spring," is nothing, or rather does not exist, unless it be as a chemical phenomenon. It is on the right of the road from Hamilton to St. Catharines, near the Red House. When I went to visit it I found neither ice nor water, but a great heap of large rocks which had fallen from the mountain and been stopped by a small ledge. These rocks are sandy, and would be very good for filters, such as are used in Paris by every person who wishes to have the Seine water pure for his table. At this place the rocks form a pretty deep cavern. From the month of July to the month of October it contains nothing but moisture. About the middle of October the water coming from the mountain gathers and fills a cavity which contains a half-litre, (a little more than a pint). This water remains there the whole winter. It is a known fact that in this season the temperature of caverns does not become lower. As the water remains then perfectly quiet, not even disturbed by the wind, and having no foreign substances suspended in it, it may remain without freezing even in a temperature of five degrees below the freezing point. In the months of March, April and May, the snow in melting absorbs a quantity of caloric, and thereby cools the neighbouring bodies ; then the May and June sun causes a certain quantity of water to evaporate, which oozing out slowly through the rocks, diminishes or lowers the temperature, so that the second drop congeals, and this is the reason why, in the warm season, a small icicle may be seen and a little congealed water in the cavity below, and that there is only water in winter. It is on the same principle that the Egyptians cool water for their use by exposing their jars when filled to the rays of the sun. But about the months of August and September, the heat being very strong and the moisture of the rock being dried up, no water is left to ooze out ; then

nothing remains to be turned into ice, and there is only the moisture which exists in all caverns. If my explanation be erroneous, what then is the cause of the formation of the ice? Since ice is formed, there must first have been water, and it is well known that the falling of the temperature causes ice to be formed. If my explanation were contested, recourse must be had to some chemical process—what would this process be? It is known that the temperature may be made to fall 40° below the freezing point by the great dissolving power of a salt, which abruptly diminishes the temperature and thereby produces ice. The spring must therefore first be in a liquid state, and the salt be at its surface; for if it is at the bottom, the ice being formed cannot ooze out through the rock, or run without being turned into water, and be converted into ice afterwards. Then I do not see by what process the ice is to be formed. I think that this name, given by persons who know nothing of science ought not to be adopted, as it may be incompatible with scientific observations, and create a belief in the existence of something marvellous, where there is only a physical phenomenon. Travellers have found what they pretend to be an ice-spring in Siberia, but as they have not given sufficient explanations, I could not ascertain whether there exists any identity between them in a physical or chemical point of view.

ST. CATHERINES.

Six miles from St. Catherines to the south, of the left of the great canal which unites Lake Erie with Lake Ontario, there is a mineral spring on the mountain from which the stones are taken for the construction of the locks. It is magnesian, but contains carbonate of lime, and disengages a very slight quantity of hydro-sulphuric gas; its specific gravity is 1.0304. The soil is calcareous and filled with fossil substances.

In the village of St. Catherines, on the border of the canal, there is a saline spring which contains lime in abundance; this takes away a great deal of its value for the manufacture of salt, especially as it contains also magnesia. This spring ascends through forty feet depth of earth, and (according to information derived from the proprietor) 407 in the rock. The

proprietor uses it for salting fish, and for baths. I think this salting cannot be very advantageous to the consumer. It might be turned to greater advantage still, by separating the salt of lime and of magnesia from the muriate of soda, by taking advantage of the salts being soluble at different degrees of temperature, and where fuel is expensive, by using the strength of the solar rays. But in the present state in which this spring exists, it would, in a commercial point of view be very difficult for the proprietor to keep up with the price current with advantage. There is a ferruginous mineral spring in the lock at the entrance of Lake Ontario. This spring might have been very useful in medicine, as it is ferruginous and magnesian, but as it is in the centre of the lock it is almost certain that it can be made no use of.

There is now nothing left for me but to describe the Niagara Spring above the great Falls. It is called the "Burning Spring," on account of the carburetted hydrogen which it contains. This spring is of no greater value than any other cold spring. The water is calcareous; it proceeds from calcareous rocks impregnated with hydrate of iron, which is not, however, in solution in the water. These gases can only proceed from the decomposition of the organic substances existing at different depths in the soil.

I will now give the description of a mineral spring containing bitumen and petroleum, and which Mr. Logan found in his first trip to Gaspé. According to the specimen he gave me, this spring is composed of creosote, naphtha, petroleum, bitumen and *paraffine* or *suif de montagne*. By means of a chemical process a substance can be formed having a strong smell of musk, and imitating that which comes from China. I think a description of this kind of spring which is found in foreign countries, will be interesting, as I am not acquainted with that of Gaspé; it is of the greatest importance to have more details on this spring, as it may conduce to other researches very useful for the country and for science. These springs of naphtha and petroleum exist in Persia, on the northern side of the Caspian Sea, at Baku near Derbent. The soil is argillaceous marl impregnated with naphtha. They may also be

found in the Birinese Territory. The Town of Bainanghong is the centre of a small District which contains more than five hundred of these springs. The soil is sandy clay, which lies over alternate beds of sandstone and hardened clay. Underneath is a thick bed of argillaceous shale of a pale blue, which forms part of the pit-coal strata, and this argillaceous shale rests immediately on the pit-coal, which is impregnated with petroleum. At Coalbrookdale in England, there is a similar spring of petroleum which originates in a bed of pitcoal. Near the Cape Verd Island large masses of petroleum have been seen floating on the surface of the sea. Petroleum has been found in the River Thames, near Lake Erie. It has also been found on Mount Osmund in Dalecarlia, in Sweden, but has since disappeared. The most remarkable feature in this appearance of petroleum, is that the country consists of transition limestone, surrounded by granite. It is then probable that the petroleum owes its origin to a bed of aluminous shale on which the limestone of the Osmund Mountain probably rests. These springs exist also in France, not far from coal-pits. As to the soil of that part of Gaspé where the above mentioned spring is found, having nothing to guide me but the specimen furnished by Mr. Logan, I can make no comparison with what is known in the other parts of the world, and thereby facilitate the theory of the formation of creosote. There are several circumstances to be considered, which are of the greatest importance. If the Gaspé spring comes from pit-coal, where is the bed of coal from which it originates? Can it be accidental like that of Sweden? This might be ascertained from the Indians of the place, and whether it is of old date or of very recent existence; this would enable me better to explain the theory of the formation of creosote in this kind of spring. And if this spring resembles those of Asia and of Europe, would it not be a sign of the presence of coal. In many points of view this spring is remarkable and may be of great value to the country; for it may furnish naphtha in abundance (more than 60 per cent.) which is very useful for light, varnish, mineral tar for ships, and creosote, which, if it can be obtained in any great quantity, will be of real advantage. Until now creosote

had been obtained by treating substances derived from the distilling of wood with phosphoric acid. This is a very expensive process. Creosote is considered as an artificial substance ; I will therefore refrain from giving the theory of its formation in this spring, as it requires more delicate experiments than my laboratory will permit me to make for the present. I had forgotten to say that the above mentioned spring, from the specimen furnished to me, contains creosote which might be procured with advantage, especially if the spring be abundant. As it is very reasonable to suppose that these springs must exist in various places, their value for industrial purposes, commerce and the arts, may be very great. The scarcity of creosote, and its high price, prevent its being used in manufactures, especially for the preservation of wood. In England, where wood is so precious, it is found advantageous to use corrosive sublimate, notwithstanding its exorbitant price, on account of the value of the mercury. If therefore one knew how to turn to advantage these natural resources, and knew the real value of this spring, that is, in how many places it may be found, how much it can furnish per hour, what is its geological position, whether it proceeds from shale or from a bed of pit-coal, or since there is such a spring, whether it is not a sign of the existence of pit-coal as in other countries ; in fine, its relation to the carboniferous substances found in the shale clefts between Gaspé and Cape Chat. From my observations on the spot itself, I found it to contain all the characteristics of pit-coal.

It is possible, and very probable, that the science of chemistry will gain something by these data ; for there may be means for extracting or facilitating the preparation of creosote so useful for medical and industrial purposes. I have no doubt that the country contains more than one person who will not hesitate to dedicate his money and time to the general advantage.

I can say nothing of the oil spring in the River Thames near Lake Erie, as my instructions did not permit me to pass the limits assigned to me ; but I consider it as indispensable to mention it here, for springs of that nature are really valuable to the country.

E. S. DE ROTTERMUND,
Chemist.

No. 6.—*Letter from the Provincial Secretary to the Provincial Geologist.*

(Copy.)

SECRETARY'S OFFICE,
MONTREAL, 22nd April, 1846

SIR,—With reference to your letter of the 17th instant, in answer to mine of the 16th, on the subject of the Salary due to Mr. de Rottermund, late Chemical Assistant to the Geological Survey of this Province, I am commanded by the Administrator of the Government to request you will acquaint me for His Excellency's information, whether in your opinion there is any objection to paying that gentleman his salary up to the first of March last.

I have the honour, &c.

(Signed,)

D. DALY,

W. E. Logan, Esquire,
Provincial Geologist.

P. S.—I have the honor to transmit to you herewith Mr. de Rottermund's Report alluded to in your communication which has just been received.

No. 7.—*Letter from the Provincial Geologist to the Provincial Secretary.*

MONTREAL 23rd April 1846.

SIR,—I have the honor to acknowledge the receipt of your communication of yesterday's date, requesting me to acquaint you for the information of His Excellency the Administrator of the Government, whether, in my opinion, there is any objection to paying Mr. de Rottermund, late Chemical Assistant to the Geological Survey of the Province, his salary to the 1st March last: and at the same time transmitting me a Report from Mr. de Rottermund.

I have to request you will do me the favor to represent to His Excellency, that Mr. de Rottermund's principal duty as Chemical Assistant to the Survey, was to make quantitative analyses of such substances as were collected on behalf of the investiga-

tion, and came before him in his official capacity. Until a list of the substances analysed with their quantitative constituents shall have been received, I am wholly destitute of the means of knowing what work has been performed in the laboratory, and to what amount of salary Mr. de Rottermund may be entitled.

The document you have done me the honor to transmit to me contains no analyses of the kind, and the list that is required would be merely a simple copy of the register which no doubt has been kept in the office, and in which the substances analysed have been noted, and their constituents entered as ascertained,—a list that could be made out in a few hours.

I have the honor to be, Sir,

Your most obedient servant,

W. E. LOGAN,
Provincial Geologist.

Honble. D. Daly,
Provincial Secretary,
&c. &c. &c.

No. 8.—*Letter from Mr E. S. de Rottermund to the Provincial Secretary.*

(Translation.)

MONTREAL, 23rd April, 1846.

SIR,—I take the liberty of addressing you my answer to the letter in which Mr. Logan thinks that my Report is not sufficient. If the Director of the Geological Survey were acquainted with the science of chemistry, I would not have been obliged to make this explanation. For, in the first place, he would have tried to ascertain whether I had in my laboratory the means of making correct experiments as regards quantitative researches. The confidence which the Government have placed in me by appointing me to this Office, has not allowed me to lead it into error by giving the results of my observations in figures:—let Mr. Logan be kind enough to look again at his Report of the two last years, and he will see that *he* has not been able to come to any correct conclusion for want of sufficient data, as he says himself. The science of chemistry has also its difficulties. It must be remarked that

Mr. Logan had every assistance he wished for,—while I was alone, without any assistance whatever. My labors, imperfect as they are in Mr. Logan's opinion, offer already a great many advantages to the country. I have discovered the presence of two substances of the greatest utility for humanity, as several persons have already been cured; for commerce, as several speculators have made offers of considerable sums to Government; for science, as these substances were unknown to the scientific world. The Government will be pleased to remark that when I arrived here, several persons, and even Mr. Logan himself, thought that yellow hydrochloric acid was good for analyses: there were also several other circumstances which I need not mention here. The ill-timed pretensions of Mr. Logan, who wanted to order me to keep a book in which I would have had to mark down when I began and when I finished an analyses, prove how much he is a stranger to this branch of the sciences; it is exactly the same thing as if a chemist, who being Director of the Survey, would ask a geologist when he began to clean a fossil and when he ascertained its name. I have never refused to assist Mr. Logan, the Geologist, and I often gave him information which he required on several very important points. As to the analyses which he says I have not made, I will answer, that having more important ones to make, I put off the others to a later period; for of what use is the chemical analysis of the lithographic stone to Mr. Logan,—it is its mechanical state, or its molecular arrangement which is necessary and essential; this may apply to other questions of the same nature.

I am ready to answer every objection Mr. Logan would like to make, to prove that he has done wrong in wishing to interfere with a branch of the sciences which he knows nothing about, and which was only given to him to assist him in researches which might be useful to the country, and not to be subject to his whims.

I have the honor to be,

Your most obedient servant,

E. S. ROTTERMUND.

Honble. D. Daly,
Provincial Secretary.

No. 9 *Statement of the Provincial Geologist.*

Statement in reference to the late Chemical Assistant to the Geological Survey of the Province.

Understanding that my late Chemical Assistant, Mr. E. S. de Rottermund, has applied to the Government to be reinstated in the Office which he recently resigned in connexion with the Geological Survey of the Province, I consider it right to place before the Government the following facts :—

Before the Government determined to permit me to appoint a Chemical Assistant, I was required to state for the information of His Excellency the Governor General (Lord Metcalfe) what his duties were to be ; and on intimating to Mr. de Rottermund his nomination to the Office, informed him, as will be seen by a copy of my letter to him of the 2nd May, 1845, (No. 1.) what those duties were, in the same terms in which they had been detailed to the Government.

On my departure from Montreal in June last, on my exploration of the valley of the Ottawa, forty-six specimens were placed in charge of Mr. de Rottermund for quantitative analyses ; and he was instructed in addition, to visit various mineral springs, for the purpose of collecting and analysing quantitatively specimens of them.

On my return from the Ottawa towards the end of November last, I found the whole forty-six specimens mentioned, exactly in the same position in which they had been left by me—not one of them analysed. I was informed by Mr. de Rottermund, that he had visited various mineral springs and analysed them. But I heard also from various quarters that while Mr. de Rottermund had been in Montreal, he had very frequently been absent from the duties of his office. I became, therefore, very anxious to know what work had been performed, and I intimated to him, on the 10th December, that I was desirous of seeing a list of his analyses, stating to him at the same time my intention to keep a regular register (nearly similar to one I had, before my departure, requested him to keep) of all the specimens analysed with their constituents. He informed me he had kept such a register, but it was not produced.

About the 18th December, Mr. de Rottermund verbally asked me for permission to absent himself from his duties for ten days. In reply, I verbally informed him to the effect, that I was fearful less time had been spent in the laboratory than the interests of the Survey required. On the 20th, he made a written application (No. 2.) for leave of absence, on the plea of urgent private business; and on the same day I gave him a written permission requesting from him in the same letter, a list of the analyses made, as will be seen by the copy of my letter of that date. (No. 3.)

Shortly after his return to town, Mr. de Rottermund intimated to me that the pressure of his family affairs disabled him from attending to the duties of his office; that he had made arrangements, or was making arrangements for entering into business connected with his family affairs, and that it was, therefore, his determination to resign his situation in connexion with the Government. Very shortly after this he wholly discontinued his attendance in the laboratory.

I subsequently pressed him, both verbally and in writing, for a list of the analyses which he gave me to understand he had performed, as will be seen by my letters of the 2nd and 31st March. (Nos. 4. and 7.)

That Mr. de Rottermund knew perfectly well what was meant by this list, is evident from the fact, that on Wednesday the , he verbally promised to give me a copy of it, stating at the same time that he could not let me have it on Thursday, because he was to dine with His Excellency the Administrator of the Government, but that he would let me have it on Friday. He subsequently appeared to confound this list with what he calls his intended Report, as if it were a general Report I was so anxious to receive; whereas what I wished for, was a simple list of the substances analysed with their constituents quantitatively stated, being a mere copy of the book it was his duty to keep, which could have been made in a few hours. The distinction is sufficiently indicated in my letter to Mr. de Rottermund of the 31st March. (No. 7.)

In this letter I drew Mr. de Rottermund's attention to the circumstance, that any Report on the labors of the laboratory was

to be communicated and addressed to me as Director of the Survey, as mentioned in my letter to him of the 2nd May, 1845, (No. 1.) in which his duties were pointed out. A document, however, has since been sent by Mr. de Rottermund, to the Provincial Secretary, who has done me the favor to transmit it to me. It is addressed to nobody, and though it is possible Mr. de Rottermund, in passing me by, may have intended a discourtesy, I should be inclined to smile at the matter, if I did not gather from the document the melancholy fact that it does not contain one single quantitative analyses; and without the performance of such, a chemical assistant is of little or no use.

By Mr. de Rottermund's letter to me of the 31st March, (No. 6.) it will be seen that he relinquished all claim to his salary for the quarter ending that day; but without any intimation to me, be subsequently applied to the Government for it. And on a reference from the Provincial Secretary's Office, on the 16th and 22nd April, in regard to the amount of salary Mr. de Rottermund was entitled to, I was under the necessity of intimating that I could not state what amount might be due to him without the list of analyses which I have so much desired to obtain. It has, however, since come to my knowledge, that Mr. de Rottermund has been paid up to the 26th February, without, however, any sanction emanating from me. Mr. de Rottermund cannot state that the analyses have not been performed in consequence of defective apparatus. All the apparatus he required was ordered. The larger part of it has been paid out of my own private funds, and he has frequently stated to me it was sufficient for all ordinary qualitative and quantitative analyses.

Mr. de Rottermund's conduct in the whole of this matter has appeared to me very extraordinary, and I have been in doubt whether he has wholly neglected his work, and made no quantitative analyses; or whether he has deliberately retained in his own possession the results of his labors, which of course are the property of the Government.

If Mr. de Rottermund's connexion with the Survey as Chemical Assistant had been continued a little longer, his negligence

in the performance of his duties, so soon as I should have had clear proofs of it before me, would immediately have become the subject of a formal complaint, with a view to his removal. But his resignation, at the moment it occurred, left me as my only course, to attempt to get from him how I could, a list of whatever analyses he might have performed. In this attempt I have not succeeded, and being deprived, by the payment of his salary up to the 26th February, of the pecuniary hold, by means of which I had some hope of obtaining the list of his analyses, (for I am inclined to think there is one) the Survey will be injured to the extent of the time and expenditure necessary to perform the Chemical Assistant's work over again.

The present statement is made in order that the Government in whatever it may determine in the case, may not act without a full knowledge of the facts. But I consider it due to myself under the circumstances which I have detailed to say, that it is impossible for me to place any further confidence in Mr. de Rottermund's diligence or discretion, and that his re-appointment in any relation to the office he has resigned, would be injurious to the Survey I have the honor to direct, and for the proper conduct of which I am responsible.

W. E. LOGAN,
Provincial Geologist.

Montreal, 20th May, 1846.

No. 1.

(Copy.)

(*Mr. Logan to Mr. de Rottermund.*)

MONTREAL, 20th December, 1845.

MY DEAR SIR—It gives me much satisfaction to inform you, that the Provincial Secretary has communicated to me His Excellency the Governor General's pleasure to direct your appointment to the office of Chemical Assistant to the geological Survey of the Province, at a salary of £300 per annum.

The duties assigned to your office are to make qualitative and quantitative analyses of the various substances collected on

the Survey, whether they be minerals, mineral waters, or soils, and you will have, as occasion serves, personally to inspect any mineral springs likely to be of medicinal importance, for the purpose of determining the quantity and kind of gases that may issue with the waters from their sources, it being next to impossible to forward specimens of these gases to a distance. An additional duty will be annually, so soon after the close of the autumn as circumstances will permit, and not later than the 1st of May, to frame a Report communicating to me, as Director of the Survey, the result of your researches, and pointing out as far as you can, the application of the substances you have examined to the useful arts and purposes of life.

In the performance of these duties confidence is entertained in your skill, and no doubt is felt that you will exert not only that reasonable diligence which is implied in accepting a task requiring constancy and attention, and whose results are of importance to the community; but that more elevated application by which men of liberal mind, distinguish their occupation when they pursue science for her own sake, and feel that their successful efforts bring their own reward.

I am, my dear Sir,
Very truly yours,

(Signed,) W. E. LOGAN.

E. S. de Rottermund, Esquire.

No. 2.

(Translation.)

(*Mr. de Rottermund to Mr. Logan.*)

20th December, 1845.

SIR,—I take the liberty of asking you to grant me leave of absence for ten days, for family matters.

I have the honor to be Sir,

Your very obedient servant,

(Signed,) E. S. De ROTTERMUND.

Mr. W. E. Logan.

No. 3.

(Mr. Logan to Mr. de Rottermund.)

MONTREAL, 20th December, 1845.

MY DEAR SIR,—You are at liberty to absent yourself from the duties of your office for ten days, agreeably to the request in your letter of this morning. In acceding to your wish, I am persuaded it is the urgency of the affairs calling you away, which induces you to apply for leave of absence, as I recollect with satisfaction the strong determination you verbally expressed to me before my departure on my exploration for the season, of letting no opportunity escape to make up for the very considerable loss of time which circumstances had occasioned previously.

I have to request attention to the enclosed memorandum. It is my wish to commence the book alluded to in it with a list of the substances already analysed; and I shall feel obliged by your furnishing me with one either before your departure, or immediately on your return, as most suitable with your convenience.

I am, my dear Sir,

Yours very truly,

(Signed,)

W. E. LOGAN.

E. S. de Rottermund, Esquire.

 (Enclosure.)

MEMORANDUM.

It is my intention to keep a book in which is to be regularly entered a list of the various specimens and materials of whatsoever description that are in charge of the Survey, and of which intimation has been given to the Chemical Assistant that an analyses is to be made. The specimens are to be numbered in succession, and in the book in question are to be registered the number of the specimen, the date on which it is indicated for analyses; the person or persons from whom it is received; a succinct general description of its external appearance and character; its locality; the date at which the said analyses is finished; and the constituents of the mineral or material.

If there is a sufficient quantity of the mineral or material, a duplicate specimen of it is to be retained and ticketed, and if a mineral, a duplicate of the portion submitted to analyses after being reduced to powder ; the last to be kept in a phial, corked, sealed and ticketed.

(Signed,)

W. E. L.

Montreal, 10th December, 1845.

No. 4.

(*Mr. Logan to Mr. de Rottermund.*)

MONTREAL, 2nd March, 1846.

MY DEAR SIR,—It has perhaps escaped your memory that on the 20th December I requested you to let me have a list of the specimens which you analysed for the Geological Survey of the Province, from your first connexion with it. I am sorry to say I have been inconvenienced by the want of it, and I hope you will let me have it with as little delay as possible.

You will oblige me by letting me know in writing, for the regulation of the pecuniary details and other arrangements of my department, from what time I am to consider that your connexion with the Government has ceased ; and I will thank you also to let me have the key of the laboratory, which by mistake you have taken with you.

I am, my dear Sir,

Very truly yours,

(Signed,)

W. E. LOGAN.

E. S. de Rottermund, Esquire.

No. 5.

(Copy.)

(*Mr. Logan to Mr. de Rottermund.*)

MONTREAL, 30th March, 1846.

SIR,—The verbal intimation you gave me about the middle of January, that it was your intention to relinquish your connexion with the Government as Chemical Assistant to the

Geological Survey of the Province, renders it necessary that I should, without further delay, place before His Excellency the Administrator of the Government, the expediency of making such arrangements in regard to the Survey, as the circumstances may require ; and not having received from you the communication you were informed it was my desire to possess, in respect to the time from which the relinquishment of your office was to date, I shall, unless you favor me with an immediate reply, be compelled to act without it, and to consider that I am at liberty to represent that your connexion with the Government ceased from the time you discontinued attendance to your official duties in the laboratory.

I have the honor to be, Sir,

Your most obedient servant,

(Signed,)

W. E. LOGAN,

Provincial Geologist.

No. 6.

(Translation.)

(*Mr. de Rottermund to Mr. Logan.*)

SIR,—In your letter of the 30th of March, you ask me to tell you until what period I consider myself to have been in the service of the Government. I have told you *vivâ voce*, that family matters are so serious and of so much consequence, that I have not been able to give all my time to the service of the Government. The very little time which I could devote to that service within the last few days enables me to state that I have not the slightest idea of claiming my salary ; and my Report, which I wish to present to His Excellency through you, will prove to every one who wishes to take an interest in it, that my services are of real use to the country, as well in a scientific and commercial, as in a mechanical point of view ; and although the state of my affairs has lately obliged me to give them an almost absolute attention, I am perfectly confident that I have done much good to the country by my labours, as

you will be able to judge yourself on examining my Report on my expedition.

I have the honor to be,

Your most humble and obedient,

(Signed,)

E. S. DE ROTTERMUND.

Mr. W. E. Logan.

No. 7.

(Copy.)

(*Mr. Logan to Mr. de Rottermund.*)

MONTREAL, 31st March, 1846.

SIR,—I have the honor to acknowledge the receipt of your communication of this date, by which I understand that in resigning your appointment as Chemical Assistant to the Geological Survey of the Province, you do not claim of you salary for the last quarter, beyond such an amount as may be considered just in proportion to the time you have devoted to the duties of your Office.

Any Report you have to make on the labors connected with the laboratory, you will be pleased to remark is to be communicated and addressed to me as Director of the Survey as mentioned in the letter I had the honor of addressing you on the 2nd May, 1845, in which your duties were pointed out. This I now state in order that no error may be made in the form of the Report. In framing it, you are at liberty to take full time to perfect it, but in the mean while I am desirous of receiving with as little delay as possible, a list of the substances analysed, with their constituents, in order that, as Director of the Survey, I may avail myself of the information it may contain in arranging the details of my department.

I have the honor to be, Sir,

Your most obedient servant,

(Signed,)

W. E. LOGAN,
Provincial Geologist.

E. S. de Rottermund, Esquire.

No. 8.

(Copy.)

(Mr. Logan to Mr. de Rottermund.)

MONTREAL, 3rd April, 1846.

MY DEAR SIR,—It will be necessary for you, as soon as you possibly can, to separate and remove such things in the laboratory as belong to you from such as appertain to me or the Government, as it is probable I shall very shortly require the room for the purposes of the Survey.

As I shall be soon preparing to leave town for the season, you will oblige me by returning to me such books or maps as you may have borrowed from me, being desirous of putting my house in order before I start.

I am, my dear Sir,

Very truly yours,

(Signed,) W. E. LOGAN.

E. S. de Rottermund, Esquire.

No. 9.

(Translation.)

(Mr. de Rottermund to Mr. Logan.)

SIR,—In answer to your letter of the 3rd of April, I have the honor to remark, that the room of which I have the key in my possession, contains nothing but my Library and that cannot interfere with your labors ; but if it be possible, will you be kind enough to leave them remain for a few days, as the crossing is very bad, and I will not be able to cross on the ice. I will be back again in a few days, and I will give you everything you require.

I have the honor to be,

Your most obedient servant,

(Signed,) E. S. De ROTTERMUND.

Mr. W. E. Logan.

Excuse the form of this letter, as I have no other paper, and am on the eve of my departure.

No. 10.

(Copy.)

*(Mr. Logan to Mr. de Rottermund.)*10 LITTLE ST. JAMES STREET,
13th May, 1846.

DEAR SIR,—I shall feel much obliged to you, if you will have the kindness to remove your books and papers from the room they occupy in the laboratory, in order that no difficulty in regard to them may be experienced during my absence on my summer's exploration.

I will thank you also to return me such books of mine as you have borrowed from me, and particularly a map on which some portion of the geology of Canada was represented.

I am, my dear Sir,

Very truly yours,

(Signed,) W. E. LOGAN.

E. S. de Rottermund, Esquire.

No. 11.

(Translation.)

(Mr. de Rottermund to Mr. Logan.)

MR. LOGAN,—I have received your letter, in which you desire me to take my library from the house. I have given orders to have a box made, and as soon as it will be ready, I will give up the room you seem so much to wish for. But if you think your prerogative to be affected, I will ask permission of the Executive Council to retain the room for a short time.

I will not forget to return you your books, which I have at home; I do not know if you are aware that all the Brantford water is mine, and belongs to me by right, and not to the Geological Survey, as I am ready to prove to you, and for that reason I alone have any right to give it away. You will oblige me by allowing the bearer of this to take away two large jars

which I promised to give to sick persons to whom it has been recommended by their physicians.

Receive the assurance of the profound consideration,

With which,

I am, Mr. Logan,

Your most humble servant,

(Signed,) E. S. De ROTTERMUND.

No. 12.

(*Mr. Logan to Mr. de Rottermund.*)

(Copy.)

10, LITTLE ST. JAMES STREET,
13th May, 1846.

SIR,—I have the honor to acknowledge the receipt of your note of this morning, and in reply I beg to say, I certainly was not aware that all the water of Brantford Spring on the premises of the Survey was your property, particularly as expenses on the water of this and other mineral springs have been paid out of the funds of the Survey, and no quantitative analyses that I know of have resulted.

I take your word, however, for the fact, and as you seem to consider the ownership of it of some consequence, which I do not, I am happy in having saved the chief part of it from destruction by the frost last winter during your absence, and I now send you not only the two jars you ask for, but all that I can find of it. At the request of * * * a *pint* of it was sent by me to * * * *, to whom, I some how or other was given to understand you had promised it; but if it is your wish I will take the earliest opportunity of replacing it by a fresh supply which there will be no difficulty in obtaining.

I should feel obliged if you will inform me whether there are any other samples or specimens on the premises that do not belong to the Survey, as it would prevent mistakes if they were removed also, and at once. I shall be happy to supply you with cases to hold your books and papers.

I have the honor to be, Sir,

Your most obedient servant,

(Signed,)

W. E. LOGAN,
Provincial Geologist.

E. S. de Rottermund, Esquire.

CRITICAL REMARKS ON THE LABOURS OF
E. S. DE ROTTERMUND, ESQ.,

LATE CHEMIST TO THE PROVINCIAL GEOLOGICAL SURVEY.

BY H. CROFT, Esq., Prof. of Chemistry, King's College, Toronto.

*To the Editor of the British American Journal.**

Some time since you were kind enough to publish in your excellent Journal (Vol. II. No. 2) some of my notes respecting the Tuscarora Sour Spring, near Brantford, in which I announced the very interesting fact of its containing free sulphuric acid; but the examination was very imperfect, partly from my want of leisure, but more from lack of material. In a note appended thereto, you mentioned that Mr. De Rottermund had discovered antimony in the same spring; and, in my letter of the 12th June, published in the August number, I fully proved the incorrectness of his analysis, or (to speak more correctly) of his statement, for I cannot believe that he ever made any analysis of the water.

In my first paper, I mentioned that it was my intention to proceed during the summer vacation to the spot, and thoroughly examine all the circumstances connected with this very curious spring, which may properly be called one of the wonders of Canada. I was prevented from so doing by untoward circumstances; and, as I wished my next communication on the subject to be a complete and final one, I should not as yet have put pen to paper concerning it, had it not been for the recent appearance of a pamphlet, entitled, "Report of E. S. De Rottermund, Esq., Chemical Assistant to the Geological Survey of the Province,—*Printed by order of the Legislative Assembly*,"†—in which this spring and its constituents are mentioned.

* Published, March, 1847.

† In justice to Mr. Logan, the talented gentleman occupying the distinguished position of Provincial Geologist, we must observe, that the report of Mr. De Rottermund was not printed with his sanction, nor has he anything, whatever, to do with its appearance.—Ed.

Mr. De R. seems to have obtained results widely differing from mine; and, as my character as an analyst must fall to the ground if his statements should be proved to be correct, I have no hesitation in requesting you to insert the following critical remarks on his assertions.

I shall first refute Mr. De R.'s statement contained in that part of the pamphlet most interesting to myself, and shall afterwards take the liberty of making a few remarks on the other portions of this highly amusing and most extraordinary publication.

At page 4, Mr. De R. says, "For the same reason, I have called the acid springs of Brantford antimoni-ferruginous, on account of the presence of that substance (Query, what substance—antimoni-ferruginous?) only recently found in mineral waters; and because it is one of the most remarkable substances as a remedy for several diseases."

Your medical readers who may be fond of *materia medica* will no doubt be glad to add this new substance antimoni-ferruginous to their Pharmacopœas, but I doubt whether any one besides Mr. De R. will ever find it in the Tuscarora Spring.

In the succeeding sentences, Mr. De R. objects to its being called an acid spring, and desires that it may be known by the above name proper to its composition. As I have shown it to be strongly acid, from the presence of free oil of vitriol, I do not see any objection to the name, especially as the carbonic acid springs are generally called "carbonated waters."

At page 10, Mr. De R. enumerates the substances contained in the spring, viz., hydrosulphuric and carbonic gas, (I quote his words), sulphate of protoxide of iron, sulphate of alumina and potash, chloride of antimony, chloride of zinc, sulphate of magnesia, and of lime, resinous substances, and vegetable albumen; from which no one would conclude that there is any free sulphuric acid present at all.

Mr. De R. speaks of three or more springs. That which I formerly examined was from the middle spring. Its specific gravity was 1.0038; it contained peroxide of iron, arising probably from its having been long kept. *It contains no antimony*, as I have again proved by new experiments. Some time since,

through the kindness of Mr. William Boulton, I was put in possession of three small bottles of the water from the north, south, and middle springs. They were well sealed; but the water contained no trace of hydrosulphuric acid, which, however, does not prove that this gas may not be present in the fresh water.

The water from the Middle Spring had a specific gravity of 1.0037 at 50 ° Faht., and contained no antimony.

“ from the North Spring, spec. grav. 1.0030—no antimony.
 “ “ South “ “ 1.0060 “

Mr. De Rottermund says the water contains zinc. Water from each of the three bottles was treated with ammonia in excess—the filtered solution treated with hydrosulphuret of ammonium—

Middle Spring—contains no zinc.

North “ “ “

South “ “ “

Mr. De R. says the water contains chlorine. Water from each of the bottles was treated with nitrate of silver—

Middle Spring— a scarcely preceptible opacity,
 hence little or no chlorine.

North Spring—no change, hence no chlorine.

South Spring “ “ “

The water from all the three springs contains protoxide of iron, as is shown by the greenish precipitate caused by ammonia; but the south spring contains the smallest quantity of protoxide, the precipitate consisting almost entirely of peroxide.

Several ounces of water from the middle spring were boiled with nitric acid and precipitated with ammonia; the precipitate washed and digested with a solution of caustic potash, the filtered solution neutralized with hydrochloric acid and precipitated with ammonia,—a slight trace of alumina was found. In my first analysis I found peroxide, and not protoxide of iron, which was most probably caused by the oxidation of the protoxide from long keeping. This water was clear and colourless, while the specimens given to me by Mr. Boulton were of a yellowish colour.

I have shown in my first paper that the spring contains lime and magnesia; these were removed (after the separation of iron and alumina) by the well known process of precipitation by acetate of baryta, &c., &c. The heated residue boiled with water, the solution filtered, evaporated to a syrup, mixed with alcohol, and inflamed, communicated a slight tinge of yellow to the flame, showing the presence of soda; the residue dissolved in a very small quantity of water, and treated with bichloride of platinum, gave a yellow precipitate indicating potash.

I have, therefore, shown that of the substances, (exclusive of gases) said by Mr. De R. to exist in this spring, viz., iron, alumina, potash, chlorine, antimony, zinc, magnesia, lime, resins, and albumen, three are certainly not present, viz., zinc antimony, and chlorine. That resinous substances should be present, seems to me to be utterly impossible, for any work on Chemistry will inform Mr. De R. that resins are *insoluble* in water. The presence of vegetable albumen seems equally problematical, for its sulphate is insoluble in acid solutions, and no precipitate is formed in the Tuscarora water, either by ferrocyanide of potassium, or bichloride of mercury (corrosive sublimate).

Organic substances certainly are present, for if the iron and alumina be precipitated by ammonia, and the residual solutions evaporated to dryness and heated, considerable blackening takes place; but we know that crenic and apocrenic acids are frequently found in mineral springs, (Berzelius, Hermann), and we know that these acids combine with protoxide of iron to form soluble salts, which then produce insoluble ones by their union with the peroxide; and knowing, moreover, that these or similar acids are found in rotten wood, in ochre, and in numberless putrefying vegetable productions, it is easy to account for the presence of organic matters in this water, without reference to such out-of-the-way substances as resins and vegetable albumen. Of the ten substances found, therefore, five may fairly be presumed to exist only in Mr. De R.'s imagination.

At page 10, five lines from the bottom, we read, "The earth is filled with sulphur crystallized in fine grains." I have some of the earth, which, as might naturally be supposed, exhibits

nothing of the kind. It contains iron and sulphuric acid ; but of this more on a future occasion.

At page 11, we have a very interesting statistical account of the diseases prevalent at Brantford, and of the cures effected by the water. This portion of the pamphlet I will leave to the critical acumen of your medical readers ; but I may remark that the cures are not to be ascribed to the presence of Mr. De R.'s "antimoni-ferruginous," but simply to that of free sulphuric acid, for every one, who is even as little acquainted with medicine as I am, must be aware that sulphuric acid is, or has been, employed in inflammation of the eyes, in cutaneous diseases, and in the treatment of sores.

At page 12, we have the following passage, which I am sure you will excuse my quoting at length, inasmuch as I am quite certain you wish to amuse as well as instruct your readers :—

"It may be useful to remark, that if water containing zinc and potash (!!), as well as alum combined with potash (!!), is very scarce (perfectly true), water containing antimony is still more important on account of its still greater scarcity and medicinal power. The salts of antimony are of the greatest value on account of the great difficulty of preparing them properly ; for the chloride of antimony becomes decomposed in water, while here it is produced by the presence of the acid, and of organic matter. The discovery of antimony in a mineral spring is undoubtedly an immense advantage for the science of medicine ; for the same substance prepared artificially is never so efficacious as when found in water, &c., &c., &c. I will take the liberty of explaining the theory of the formation of this antimoni-ferruginous spring, and of explaining the cause why the antimony is in solution without being decomposed or precipitated by the water. According to geological researches, it is known that this place contains a great many marshes, iron pyrites, lead ore, zinc, and antimony, as well as gypsum. Water running through iron pyrites or gypsum, by some chemical or other phenomenon, becomes decomposed, and charged with a quantity of sulphuric acid ; (the waters of this spring prove its presence in a very decided manner ;*) the water so acidu-

* Here mentioned for the first time.—H. C.

lated, passing through turf or a marsh covered with vegetation, must contain vegetable albumen which I have found, organic acids, resinous substances, &c. If the water thus charged passes afterwards through beds of ore of antimony, it is natural that it will dissolve the substances without precipitating it."

In the first place, the statement of any preparation of antimony prepared artificially not being as efficacious as when found in water, is, *in abstracto*, a decided fallacy. I believe (speaking under correction) that the idea of springs possessing peculiar virtues, not to be imitated by artificial means, is now entirely exploded. But let us look at Mr. De Rottermund's theory, and we will suppose that all the substances he requires do really exist in the neighbourhood. Did any one ever hear of water being decomposed when in contact with sulphate of lime and becoming charged with sulphuric acid? In that case half the springs known in the world should contain it. Mr. De R. very properly adds, "by some chemical or *other* phenomenon." But by means of iron pyrites, sulphuric acid might be formed: this, says our author, passes through turf or marsh covered with vegetation, and becomes charged with vegetable albumen (?), organic acids, (here mentioned for the first time), resinous substances (?), &c. &c. &c. Now, this solution, "passing through beds of ore of antimony, will naturally dissolve the substance without precipitating it;" that is to say, water containing sulphuric acid, resins, albumen, and crenic acid, will dissolve an ore of antimony (probably the sulphuret). I am afraid Mr. De Rottermund is like the facetious old gentleman who, hearing a friend cry out, "*lapsus linguæ*," when his servant let fall a boiled tongue, caused his own attendant to be equally awkward with a round of beef, and expected to gain great applause by a repetition of the witticism.

Mr. De R. has heard that when the oxide or sulphate of antimony is boiled with bitartrate of potash, the oxide is dissolved and that the precipitation of the oxide, or basic salt of antimony, by means of water, is prevented by the addition of some organic acid, and he wishes us to believe that something similar takes place in the present instance. Let him believe it who can. Besides, he need not have given himself the trouble

of accounting for the antimony, because there is none there; which puts me in mind of the twenty-fourth reason for not ringing the bells, viz., because there were no bells to ring.

In the preceding remarks I have shown the utter fallacy of Mr. De Rottermund's statements with regard to the Brantford Springs; and in my next communication I shall take the liberty of endeavouring to prove similar inaccuracy in the remaining portions of the pamphlet. I may, however, remark, that it is scarcely necessary to attempt a refutation, for Mr. De R.'s statements are unsupported by experiments, or the description of one single analysis either quantitative or qualitative. I have not found in the whole pamphlet so much as a statement regarding the specific gravities of the waters spoken of, except in one place, and in that the numbers had, I believe, been determined by other chemists.

Without some such guarantee, Mr. De Rottermund cannot expect that any one of his statements will be believed by the scientific portion of the people of Canada, although it is probable that so flourishing a report may obtain credence among those who may have as inaccurate a knowledge of chemistry as even Mr. De Rottermund himself—a portion of the community which, it is to be hoped for the credit of the country, will be found to be exceeding small.

Toronto, 9th Jan., 1847.

*To the Editor of the British American Journal.**

SIR,—A friend has this moment placed in my hands the March number of your valuable Journal of Medical and Physical Science. In it I notice some critical remarks (as they are rather amusingly and pompously termed) on my labours as Chemist to the Provincial Geological Survey, by H. Croft, Esq., Professor of Chemistry, King's College, Toronto.

As Mr. Croft is not satisfied with making some very petulant and silly allusions to his loss of *reputation*, and to my capacity as a chemist, but has also questioned, in his peculiar style, the accuracy of my *qualitative* analysis of the Tuscarora spring, it will become necessary for me to reply to his stric-

* Published, April, 1847.

tures, and to offer a few observations upon his attempts, as exhibited in the columns of your journal in its numbers of June and August, 1846, and in the last. I trust you will permit me to avail myself of your columns for that purpose. Unavoidable absence from town, and a variety of pressing occupations, will prevent my preparing anything for April; I am therefore reluctantly compelled to postpone my remarks until your May number. In the meantime, I would remind Mr. Croft, (an important truth which he seems entirely to have forgotten), that a contemptuous mode of expression is no proof either of capacity or attainments; and, moreover, that his attempts to show what ingredients *are not* to be found in the Tuscarora water, may simply illustrate the fact that under the most favourable auspices, he is of himself, Mr. Croft, Professor of Chemistry, King's College, Toronto, incapable of determining the elements of which it *is* composed. His efforts pretty plainly demonstrate this incapacity, when he is unassisted by others.

I remain &c.,

E. S. DE ROTTERMUND.

Montreal, 15th March, 1847.

REPLY TO PROFESSOR CROFT'S "CRITICAL REMARKS."

By E. S. DE ROTTERMUND, Esquire.

*To the Editor of the British American Journal.**

SIR,—I trust Mr. Croft will understand distinctly, that if I have been unwilling to notice his analysis of the Brantford waters, or to bestow any serious attention upon them, I was not deterred by any fear of entering into a scientific discussion; but simply because I observed in them errors of such magnitude, and so extraordinary, that I was disposed to regard his articles rather as the pretensions of a man desirous of exhibiting his ability to read and sign his name, than as the efforts of

* Published, May, 1847.

one capable of producing original matter, or even of copying correctly. Since, however, Mr. Croft, with significant pomposity, assails me, under his title of "Professor of King's College," I am induced, through respect for the situation he holds, to reply to his strictures. I make this observation, because I should never have considered myself obliged to notice him as a chemist; for this plain reason, that neither by his style or matter does he prove to me his right to that distinction; but most particularly the reverse by his last article, dated January 9th, and published in your March number.

Had Mr. Croft stated in his article, published in the number of your Journal for June, 1846, that he had read the description of a mineral spring analysed by Mr. Boussingault, he would have acted more judiciously than to have re-produced that analysis so incompletely. Were not the distinguished merits of Mr. Boussingault, so well and so extensively known, Mr. Croft would expose him to the reproach of being deficient in the most essential details, and those which are pre-eminently necessary in the publication of a quantitative analysis, in order that its value may be accurately appreciated. But I suspect that Mr. Croft, in his wish to imitate Mr. Boussingault, was not aware of what was necessary to render his article complete. What appears most unaccountable is, that one who styles himself "Professor of King's College," should commit a blunder so gross in regard to the use of carbonate of ammonia in determining the presence of magnesia, as would be unpardonable in a mere student of medicine. As Mr. Croft seems to be acquainted with some scientific printed authorities, if he had referred to some of those works for the use of tyros who are receiving instruction in the means of ascertaining different bodies by this or that re-agent, he would have discovered his error.

Phosphate of soda forms with magnesia, a double insoluble salt of phosphate of soda and of magnesia in a neutral solution, In acid solutions, ammonia is added to neutralise the acid, and thereby to facilitate the formation of the double phosphate. Professor Croft employs carbonate of ammonia instead of caustic ammonia. If the solution be acid, it is certain that the car-

bonate of ammonia will be decomposed, forming a new salt, with an ammonia basis ; and the carbonic acid, which becomes free, will form insoluble carbonic salts, such as the carbonate of lime, of magnesia, of alumina, and the phosphate of soda can no longer act. Mr. Croft, has, therefore, erroneously taken the carbonate of lime, or some other carbonate, for the phosphate of soda and magnesia. If Mr. Croft be a man of candor, he must admit that such a blunder is unpardonable, even in a tyro who recites his first lessons, and much more inexcusable in a "Professor," who blunders into an analysis thus—"ammonia precipitates a reddish-brown flocculent substance, sesqui-oxide of iron, and perhaps alumina." For amusement, if not for edification, I should like Mr. Croft to inform us what he means by the science of chemistry, and what is the use or object of an analysis if the chemist is to be as easily satisfied as the learned Professor. "Sesquioxide of iron, and perhaps alumina," "little or no chlorine," "little or no protoxide of iron," and so on ! This is precisely a case in which it may be safely said, that Mr. Croft is either a poor chemist, or, "perhaps," no chemist at all. He should have known that carbonate of ammonia yields a white precipitate, with salts of magnesia, barytes, strontian, manganese, alumina, zinc, antimony, lead, tin, &c. Phosphate of soda yields a white precipitate, with salts of magnesia, lithia, barytes, alumina, iron, zinc, antimony, tin, lead, &c. Now, what right has Mr. Croft to assume, or to take upon himself to state, that because he obtained a white precipitate by means of phosphate of soda (having previously added the carbonate of ammonia) he discovered the presence of magnesia, when these two re-agents yield a white precipitate, with so many others ? Who knows but Mr. Croft, instead of magnesia, saw *lithia*, or alumina, or antimony, or zinc, &c., &c. ?

Mr. Croft states the quantity of the different substances, and supposes, since Mr. Boussingault found sulphuric acid, hydrochloric acid, alumina and lime in the waters near Papayon ; and he, Mr. Croft, finding sulphate of sesqui-oxide of iron, lime, and magnesia in those of Brantford, that therefore the bed of this spring, like that of Papayon, must be volcanic. What absurdity ! Every man of common sense, not wishing to inflict

on Mr. Croft the pain of contradiction, can only be amused at seeing him hunt for an analogy between the two analyses. A compassionate shrug of the shoulders is the only argument to be made use of in such cases. But let us come to his quantitative analysis.

First, Mr. Croft should have stated the quantity of the residuum, obtained from the evaporation of a given quantity of the water, say a pint, to prove if he made any losses, and in what stage of his analysis.

In June, 1846, Mr. Croft says he found sulphuric acid, sesqui-oxide of iron, magnesia, and lime. In his communication of the month of August, he expresses his surprise at the presence of antimony in the waters of Brantford, and cites the analyses of Daubeny, Bischoff, and Ozann, to prove that that body does not exist in any mineral spring. We must presume, therefore, that the Professor denies the possibility of discoveries in chemistry, more particularly in mineral springs. We are justified in this belief, since the main ground upon which he rests his assertions is, that chemists, since the creation of the world, have not mentioned this body in any analysis of mineral waters.

In March, 1847, after Mr. Croft had perused my report, he finds, in the same mineral water, chlorine, protoxide of iron, soda, potassa, *alumina*, and "he adds that I do not mention any trace of free sulphuric acid, discovered and published by him" in June, 1840. I would ask him what process he adopted to detect its existence—more particularly, I would request him to inform me what course he took to convince himself that the sulphuric acid is or is not free? I suspect he will experience very serious difficulty to answer me these questions. I should believe him upon his word; and, as Mr Croft publishes exact numbers, we must suppose that he weighed each of these bodies in a state of perfect purity. Our entertaining any doubts upon this important point would be a direct attack upon the honour of a chemist. So much the worse for Mr. C., if he has ignorantly, incautiously ventured to substitute protoxide for sesquioxide, sulphuric acid for chlorine, phosphate of soda and magnesia for carbonate of magnesia, carbonate of alumina, of lime, &c., &c. Mr. Croft is his own accuser, and leads us to

doubt the accuracy of what he advances so lightly and with such inconsiderable precipitation.

Mr. Croft, with affected irony, reproaches me with not having positively stated that the sulphuric acid is or is not free. I must admit that I am not provided with that audacity, or blind confidence in myself, which could alone have enabled me to bring before the public as a positive fact, what, in my opinion, could be given merely as a supposition. Whatever Mr. Croft's way of thinking or acting may be, mine is, that in scientific matters, one should rather be the notary than the barrister.

To state that sulphuric acid is, or is not free, requires a quantitative analysis, the most delicate and exact that can possibly be made, for supposing that there exists a certain quantity of acid a , and a certain quantity of base b , which would saturate but half the quantity of acid a , if now in analysing a certain quantity of any substance, there is found a certain quantity of acid a , and if, instead of the quantity of base b , another quality and quantity is given without exactitude, can any one determine what is the quantity of free acid present; for protoxide is not peroxide, nor is iron, alumina, each of those bodies having different powers of saturation with the same acid. And can Mr. Croft state by what rule or theory, approved by science, he has presumed to say, that the acid is, or is not free; but an answer to every one of Mr. Croft's accusations, would be a greater task than that which a professor might find with a tyro who has taken a wrong notion in his head. Mr. Croft misconstrues the thoughts and the facts also; moreover, with a sort of self-satisfaction, he argues on a word, instead of the idea involved in the sentence.

Mr. Croft takes up my theory of the composition of the spring, in which I suppose that to have free sulphuric acid, the sulphate of lime must have been decomposed, which with other circumstances, tended to facilitate the explanation, why antimony was in a state of solution—without saying if the sulphuric acid had entirely or partly served towards the formation of other salts. Mr. Croft seems to have considered this theory in the light of an analysis, as if I could have been *under the earth*, or have judged by any other means than those which are al-

lowed to the disciples of science, viz., their intelligence based upon scientific information. If Mr. Croft is more capable, he can propound another theory. Many phenomena were at one time explained by the phlogistic theory, which has given place to the one based upon the action of oxygen. Neither theory, however, prevents mercury from becoming oxidated, by contact with air.

Mr. Croft, in June, 1846, states that the nitrate of silver did not detect anything in the waters of Brantford. He is mistaken; as there must have been formed a white precipitate of sulphate of silver. In March, 1847, he finds a white precipitate with nitrate of silver, and he takes it for chloride. He would have done better to inform us what means he adopted to separate sulphuric acid from the chlorine, in order to be certain that the precipitate obtained by the sulphate of silver was chloride and not sulphate of silver. Mr. Croft has had the goodness to admit, after reading my analysis, that he found some traces of chlorine. I thank him, also, for having found potassa, *alumina*, and soda. But this I regard as mere civility on his part, because he must permit me to doubt, if he saw these bodies by the method which he employed. He saw the yellow flame of the spirits of wine, but it was produced by organic matter, or by some other salt than pure soda, because he did not obtain it in a state of purity to render that test unexceptionable. Who knows that Mr. Croft may not also have mistaken a sulphate, phosphate, bromide or iodide, &c., for the chloride, when the solution was tested by the nitrate of silver; inasmuch as all these salts yield the same white precipitate with the nitrate of silver.

Thus, what opinion can or ought to be formed of a chemist, who, after publishing three quantitative analyses of the same mineral water in June and August, 1846, and in March, 1847, at last finds six additional bodies, and all different from the first, which he supposed to have existed exclusively there? What ought we to say of a chemist who discovers a resemblance between two analyses, in consequence of the presence of a body which exists in nearly all the most common substances? Mr. Croft, in June, 1846, publishes different quantitative analyses, in

which he states he found sulphuric acid. In the

1st Experiment, 21.670 grs.		
2d	do.	23.597 "
3d	do.	22.049 "

without stating the quantities of matter upon which he operated. He only assures us that he found 7.680 grs. in one pint of water. In the month of March, 1847, he apologises, admitting that he was mistaken, and declaring that he had not had the time to make a complete analysis, and that, in fact, he did not possess the necessary materials. If such were the case, he ought to have acted with the prudence and integrity of a man of science, and not advanced and published what he could not sustain, much less prove. Mr. Croft supposes the soil of Brantford, where the spring is situated, to be volcanic; and, singularly enough, it is the presence of sulphates of an acid character which leads him to this conclusion. If Mr. Boussingault has declared that the bed of the spring near Papayon is volcanic, his opinion rests upon the fact that he found hydrochloric acid;—hydrochloric and sulphurous acid may be indications of volcanos, but sulphates are not.

After criticising with so little gentleman-like courtesy, after having so flippantly remarked upon the labours of another chemist, acknowledged as such by authorities in science possessing rather more celebrity in the scientific world than Mr. Croft, notwithstanding his title of professor—after having bedaubed and exposed himself to ridicule, in his own ambitious but utterly worthless publications, and that by his own showing, I put it fairly to Mr. Croft whether or not he is entitled to any further reply? I would add, that were I sure that he had the intention or the will to persevere in the study of chemistry, I should earnestly advise him to continue his investigations of the Brantford waters. This spring, or in 1848 at the very latest, he will hit upon some new ingredient, which has hitherto escaped his triple and thrice contradictory analyses.

You say with truth that my report was not sanctioned by the signature of Mr. Logan. I may remark, however, that Mr. Logan, not being a chemist, could not in any way sanction that

report. Be that as it may, I am most certain that that eminent geologist, whom you justly regard as a savant of high ability, would not consent to sanction Mr. Croft's analysis, much less would he uphold the supposition that the soil of Brantford is volcanic, because it contains sulphates of iron, of lime, and of magnesia.

I should have terminated my reply to Mr. Croft, but being assailed by the *Argus*, a political, literary and scientific journal of Toronto, which so loudly applauded Mr. Croft's article against me, I feel myself bound, through respect for the press, to offer a few more observations. As I have already said in my report, and I repeat it again, I could not give to the public any quantitative analyses; for notwithstanding every care in my investigation, I had not the necessary means of making them correctly; and as I regard the word of a man of science when he advances any fact connected with his labours as most sacred, I preferred publishing the qualitative analyses only, and to keep in my own possession the quantitative; because, although they appeared to be correct, they might have been inexact, for want of the necessary apparatus.

Mr. Croft expresses his astonishment at my finding zinc and potassa in solution in the water. He makes use of the printed signs of admiration, which so well depict his thoughts. If Mr. Croft does not know the existence of double salts of sulphate of zinc and of potassa, yet as a professor he should recall to mind that the salts known under the name of halhydrates, form double salts only with salts in which the acid has lost its water. If, having the wish to attack the labours of chemists, he knew a little of the science, he would have placed the exclamation thus, zinc and lime! I would have had the trouble only to object to him that he is mistaken in his reproductions; for he would be right in saying, that sulphate of zinc cannot form a double salt with the sulphate of lime, inasmuch as both are halhydrates; but the sulphate of zinc forms a double salt with the sulphate of potassa and others not halhydrate. The sulphate of lime will form them with the same salts. And here is the formula, according to Liebig, which perhaps will serve to calm Mr. Croft:



Other chemists give seven atoms of water.

As to the antimony, I obtained it in the state of sulphuret, of an orange red colour, giving with the blow-pipe a white smoke depositing itself on the coal in a circle. And to convince Mr. Croft that I take great precautions in my analysis, I would remark, that, fearful lest the presence of the antimony might have been due to the hydro-sulphuric acid, which might have contained some traces of that metal, derived from the material from which it was obtained, I expressly prepared some sulphuret of iron myself; and when I again obtained the orange coloured precipitate, I was convinced that antimony was contained in the solution. I am free to confess the extent of my surprise at this discovery; for I was aware that that body had not been previously detected in mineral springs. But Mr. Croft would never be able to find it, even if I were to point out to him the method of doing so, inasmuch as practical skill is necessary.

As to the proof of vegetable albumen, let Mr. Croft read the analysis of the waters of Barege, and afterwards examine the waters of Caledonia, if perchance another fit of chemical analysis should seize him. He will, no doubt, suppose that there are broken eggs in the soil, if he should discover vegetable albumen. As to the solubility of resinous matter in water, Mr. Croft is nearly perfectly correct in his position. It is insoluble as iron is, as also zinc and other bodies, and which nevertheless we find in solution in many springs. But since I am compelled to adopt language more practical than scientific, in order to be more intelligible to persons unacquainted with the science, I would mention certain daily practises, adopted by manufacturers to increase the weight of their articles, to the prejudice of the public. They mix resin with potassa, which, in the form of soap, is soluble in water. Analyse such a water, and you will not be surprised to find resinous matter in it in a state of solution. Since the manufacturer, in order to carry on his trade more successfully in a commercial point of view has found out the means of overcoming or concealing the insoluble nature of resin, why should we doubt that nature, so mysterious in her existence, and frequently so in her operations which often seem incompatible to ordinary intelligences, might

not also form some combinations, which, we can reasonably suppose, would escape Mr. Croft's scientific attainments. Mr. Croft finishes his article thus: "Mr. De Rottermund cannot expect that any one of his statements will be believed by the scientific portion of the people of Canada, although it is possible that so flourishing a report may obtain credence among those who may have as inaccurate a knowledge of chemistry as even Mr. De Rottermund himself, a portion of the community which, it is to be hoped, for the credit of the country, will be found to be exceedingly small." A direct answer to this is scarcely necessary, and would be of very little importance; but I will merely take the liberty of submitting a problem which can be solved by the latter "portion of the community" just as well as by the "scientific portion of the people of Canada"—could not the combinations produced by nature be as well concealed from Mr. Croft's understanding, as are the preparations of an able artisan from the ordinary purchaser?

I prefer saying nothing to refute the assertion, that we can expect more in our laboratories than nature in her combinations, because the apothecary who has the recipe for a preparation, may defy us to prepare his ointment. His skill in turning the pestle produces that which a chemist the most expert in the theory of combinations, could not imitate, and nature possesses far greater means and secrets of preparation than the most skilful chemist. The only correction which I expect to receive in regard to my labours, is, that I may have been deceived in the state of the salts. But if that error does exist, it does not arise from any want of exactness, but from the want of those necessary means which no chemist engaged in delicate analyses, can dispense with. In the beginning I declared frankly, that I was not possessed of what was necessary. In order to determine the state of the salts, an analysis more accurate than Mr. Croft could make would be required.

Errors frequently arise from the calculations and the figures arising out of the theory of the laws of combination. Thus a man corrects himself if he sees that another chemist has seized the truth with more accuracy than he has; but on that account a man is not less a chemist. Thus M. Liebig corrects M. Du-

mas, and M. Dumas corrects M. Liebig. And it is immaterial what my labours are, and be their merits what they may, I defy Mr. Croft to make that analysis, without my shewing him at every step errors in principle, and inaccuracies; and I will prove to him that he has mistaken water for an acid, protoxide for peroxide, paper for volcanic matter, and who knows but some other substances, similar to those of the sour spring near Papayon, and others again still more remote, with which Mr. Croft would rejoice to compare them. But to bring this communication to a close, take care, Mr. Professor, not to do "as the facetious old gentleman did, who, hearing a friend cry out '*lapsus lingua*,' when his servant let fall a boiled tongue, caused his own attendant to be equally awkward with a round of beef, and expected to gain great applause by a repetition of the witticism;" the pretty story which you cite in your admirable critique, in the March number of the *British American Journal of Medical and Physical Science*, in answer to my labours.

St. Césaire, March 30, 1847.

REPLY TO MR. DEROTTERMUND.

By HENRY CROFT, Esq., Prof. of Chemistry, King's College.*

SIR,—(1) In the last number of your Journal, received yesterday, I find a letter from Mr. DeRottermund, purporting to be a reply to my criticisms on his report, published in the March number. This is probably the communication promised by Mr. DeRottermund in the number for the previous month, and in which I had fully expected to receive a complete account of Mr. DeRottermund's qualitative experiments—but in this I am most lamentably disappointed; for, as far as regards that gentleman's own researches (with the exception of one experiment) we remain exactly in *statu quo*.

By far the greater part of the letter is directed personally against me, and consists of good, strong, wholesome abuse; to which branch of polite literature I would recommend Mr. De Rottermund to direct his entire attention, for he succeeds infinitely better with that, than chemistry.

* Published, June, 1847.

I shall refrain from making any remarks on this part of his communication, and will confine myself to the scientific portions; and when I have proved Mr. DeRottermund's utter and palpable ignorance, I will leave it to your unprejudiced readers to determine how much truth there may be in the allegations advanced against me.

(2) Mr. DeRottermund's first attack on me is for not giving Boussingault's analysis of the water in Papayon in full. This was not done for two very good reasons, 1stly, Because it was entirely unnecessary to mention more than had direct reference to the subject on hand; and 2dly, Because I do not possess a full account of the analysis, having taken the numbers from Hoffman's *Physikalische Geographie*. In paragraph 13, Mr. DeRottermund says, (alluding to me,) "after having so flippantly remarked on the labours of another chemist, possessing more celebrity in the world," etc., etc. If by this Mr. DeRottermund alludes to himself, I have nothing to say; but if he refers to Boussingault, he is guilty of a mis-statement, to use the mildest word. It requires the acumen of a Mr. DeRottermund to discover how quoting from the works of a great philosopher can be accounted "remarking flippantly on his labours."

(3) Mr. DeRottermund then branches off at a tangent and attacks my use of carbonate of ammonia in testing for magnesia, "a mistake (as he calls it) unpardonable even in a student of medicine," and he then gives me a lecture upon various points of analytical chemistry, which I will now proceed to elucidate. As, however, Mr. DeRottermund requires to have conviction *forced* upon him, I have taken the trouble of referring him to certain portions in the works of Berzelius and H. Rose, hoping that the statements of the greatest chemist, and the most accurate analyst in the world, will have sufficient weight to convince even Mr. DeRottermund.

(4) "Phosphate of soda (says Mr. DeR.) forms, with a neutral solution of magnesia, a double insoluble salt of phosphate of soda and of magnesia." This salt, I must allow, is entirely unknown to me. At page 304, vol. 4, of Berzelius' *Lehrbuch der Chemie*, you will find, "phosphate of magnesia, $2 \text{MgO} + \text{P}_2\text{O}_5$, is produced when hot solutions of phosphate of soda and

sulphate of magnesia are mixed together, the salt crystalizes on cooling;" and words to a similar effect, in Rose's *Handbuch der Analytischen Chemie*, vol. 1, p. 41.

The salt mentioned by Mr. DeR. is, therefore, not formed under such circumstances, and phosphate of magnesia which is produced, is tolerably soluble in water.

"In acid solutions, ammonia is added to neutralize the acid, and facilitate the formation of the double phosphate." Now if ammonia be added, another entirely different salt is procured, viz., the ammoniaco-magnesian phosphate. Vide Rose, p. 42, and Berzelius, vol. 4, p. 305.

That either ammonia or its carbonate may be used in precipitating this latter salt, is mentioned by Rose and Berzelius in the same pages. "If in acid solutions, (says Mr. DeR.) carbonate of ammonia be used, another salt of ammonia will be formed, and the carbonic acid being set free, will produce insoluble carbonate of lime, magnesia, and alumina." In the first place, every beginner is aware that such a thing as carbonate of alumina does not exist, (Berzelius, vol. 4, p. 333;) secondly under such circumstances, carbonate of magnesia would *not* have been precipitated,—one of the fundamental rules in testing for magnesia, and of which Mr. DeRottermund may convince himself by a simple experiment, (easy to one who possesses his vaunted *practical dexterity*). Let him make a solution of any salt of magnesia, add a few drops of acid to it, and then an excess of carbonate of ammonia—he will find no precipitate, owing to the formation of a soluble double salt; and, thirdly, had Mr. DeRottermund taken the trouble of reading the experiment, he would have found that all the lime had been previously removed by oxalate of ammonia.

From this portion of the paragraph, we may deduce five conclusions with regard to Mr. DeRottermund's knowledge, which I will presently enumerate.

(5) Mr DeR. also says, "He should have known that carbonate of ammonia yields white precipitates, with salts of baryta, strontia, magnesia, alum, zinc, antimony, lead, tin, etc.; that as phosphate of soda gives white precipitates, with salts of magnesia, *lithia*, baryta, alumina, iron, zinc, antimony, tin, lead,

etc., and as he had previously added carbonate of ammonia, he might just as well have had a carbonate of lithia, alumina, etc., etc., (all the above bases,) as of magnesia."

It is scarcely necessary to remind your readers of certain facts unknown to Mr. DeRottermund, viz., that salts of baryta and strontia are decomposed by sulphates, that lithia is not precipitated by phosphate of soda alone, that the oxides of iron, aluminum, tin, lead, and antimony, are precipitated by ammonia; and salts of zinc and manganese, by oxalate of ammonia.* But we may deduce nine more conclusions from this latter portion of the paragraph, which I will now enumerate, together with those already mentioned.

1. Mr DeRottermund is ignorant of the action of phosphate of soda on salts of magnesia.
2. Do do of the action of phosphate of soda on a salt of magnesia, when ammonia or its carbonate is present.
3. Do do of the action of carbonate of ammonia on acid solutions of magnesia.
4. Do do of the non-existence of carbonate of alumina.
5. Do do of, or cannot understand, the method of separating lime from magnesia.
6. Do do of the insolubility of the sulphates of baryta and strontia.
7. Do do of the precipitation of these earths by oxalate of ammonia.
8. Do do of the precipitation of alumina by ammonia.
9. Do do of the precipitation of oxide of iron by ammonia.
10. Do do of the precipitation of oxide of lead by ammonia.
11. Do do of the precipitation of oxide of tin by ammonia.
12. Do do of the precipitation of oxide of antimony by ammonia.

* I adopted the usual plan in my analysis, first precipitating with ammonia, then with oxalate of ammonia, and afterwards with phosphate of soda and carbonate of ammonia, as is recommended in all good works on analysis; and yet Mr. DeRottermund argues, as if all the above mentioned substances might have been still present.

13. Do do of the precipitation of oxides of zinc and manganese by oxalate of ammonia.

14. Do do of the action of phosphate of soda on salts of lithia. Vide Rose, vol. 1, p. 16; Berzelius, vol. 4. p. 191.

We have here fourteen of the commonest facts, of which Mr. DeRottermund, by his reasoning, shows himself to be utterly ignorant, and were I desirous of sparing myself much useless trouble, I might here conclude; for from the above, any one can form an estimate of the reliance to be placed on his assertions.

(6) In paragraph 4, he ridicules me for saying that the bed of the Brantford spring was volcanic, because I had found some sulphates in the water; whereas Boussingault had found sulphuric and hydrochloric acid in volcanic springs. This assertion of Mr. DeRottermund contains a double misstatement,—1stly, Because I not only found sulphates, but also free sulphuric acid; and 2dly, Because I never said anything about the Brantford formation being volcanic—but expressly stated that the sulphuric acid must have been produced differently from what it is in the springs of Java and South America. The plain fact was that having found free sulphuric acid in a mineral spring, I mentioned all others that I knew of containing the same substance—to which last I have now to add a spring in the Genessee country, found to contain sulphuric acid by Professor Eaton.

(7) In paragraph 5, I am blamed for not determining the residue left on evaporating a certain quantity of the water, a process which could not be adopted as it contains free sulphuric acid; and sulphates of iron lose part of their acid with the last traces of water; a fact which should certainly not have been unknown to the late chemist to the Geological Survey.

(8) In paragraph 6, I am accused of denying the possibility of antimony being discovered in springs—whereas, I not only mentioned some chemists who had never discovered it, but also proved that it was not present in the water then in my possession, as I have since done with several other specimens.

(9) Mr. DeRottermund wants to know how I detected free sulphuric acid, and reads me a long lecture upon the way in

which it should be done, viz., by calculation. If Mr. DeRottermund had taken the trouble to read my paper, he would have found that this very process was adopted; but he does not appear to be so well off as those individuals to whom he compares me in his first paragraph, for he does not seem to be able to read, certainly not to understand a plain statement.

Let him calculate the whole of the iron as peroxide, (including the alumina, as in my first analysis;) let him, for the sake of brevity, add the magnesia to the lime, and calculate how much sulphuric acid these bases will neutralize, he will find it to be about 13 grains—the remainder is more than sufficient to supersaturate the small amount of alkali present; or, if he wishes a practical test, let him notice the violent corrosive action of the Tuscarora water on the teeth—a property which I do not believe any solution of alum, sulphate of iron, lime and magnesia would ever possess.

It is scarcely necessary for me to explain why, in my first analysis, I found peroxide of iron, and a mixture of it with the protoxide in my latter ones—I have done so already; but Mr. DeRottermund takes every opportunity of neglecting some of my statements, and perverting others, according as it suits his own convenience.

(10) Paragraph 10, I must allow, is almost beyond my comprehension. If Mr. DeRottermund wishes me to give a theory explaining the presence of zinc and antimony in the spring, I must beg to be excused, for I do not wish to attempt to account for things that cannot be found. A theory, as Mr DeRottermund says, "must be based on scientific knowledge," for which I would substitute "acquaintance with facts," a species of information in which he appears to be rather deficient.

(11) Mr. DeRottermund says, that with nitrate of silver, I should have obtained a white precipitate of sulphate of silver. Now, as this latter salt is considerably soluble in cold water, if the solution tested be dilute, no precipitate will be formed, of which he may convince himself by a simple experiment, or by reference to Berzelius, vol. 4.

(12) He says, that in March, 1847, I found a white precipitate; another perversion, as my words were, "a scarcely perceptible opacity."

He insinuates that the yellow flame of alcohol, which I supposed was produced by soda, was owing to organic substances, while, if he had been able to read, he would have found all organic substances had been previously destroyed; he requires that the soda should be separated in a state of purity before making the experiment, not knowing that if one part of soda be mixed with twenty or thirty parts of potash and burned with alcohol, the yellow flame will predominate. Mr. De Rottermund, in the same paragraph, shows that he is not aware that phosphate of silver is yellow and soluble in acids—that he is ignorant of iodide of silver not being white.

In paragraph 12, he blames me for not giving the quantities of water operated on in my analysis, while in the very line before, I had stated that the quantity in each case was one pint.

He states that I found 7·680 grains in one pint; mistaking, with singular obtuseness, the figures 7680 representing the number of grains in a pint of water, for a part of the analysis: he states that *free sulphuric acid* is present in the most common substances, (for it was the presence of this substance that rendered my analysis similar to Boussingault's;) he states that, in 1847, I found "six additional bodies, different from all those discovered at first which I had imagined to exist there exclusively," this is again a double mis-statement. In my first paper, I mentioned the possible presence of alumina and alkalis, and my inability to test for them from want of material;—the only additional substance is an organic matter; and lastly, the statement that I retracted my former statements, is as unfounded as the remainder of the 11th paragraph, the part of the 13th which asserts my analysis to have been thrice contradictory, and in fact as the greater part of Mr. DeRottermund's communication.

(13) In paragraph 17, we get the solitary experiment described. Mr. DeR. seems to take great credit to himself for the same, and in the conclusion launches out into a brilliant eulogium on his own practical dexterity. I have already stated that I performed the same experiment with four specimens of the Tuscarora water, without finding a trace of antimony—a result owing (as Mr. DeR. says) to my want of practical dexterity.

(14) In paragraph 16, Mr. De Rottermund explains the presence of zinc in the water, (as I understand him,) by the fact that a double salt of sulphate of zinc and potassa can be prepared artificially; he makes no experiment to prove its presence; he does not tell us how he discovered zinc in the first instance—but he gives us a learned disquisition on halhydrates which has nothing whatever to do with the subject.

In paragraph 18, he is very facetious about *broken eggs* in connection with *vegetable albumen*, and then proceeds to explain, or rather *not* to explain how resins got into the water. He adduces as a parallel case, (if I understand him rightly,) that soap boilers adulterate their produce with resin soaps. Most chemists are aware that some resins will unite with strong caustic alkalies, forming compounds analogous to soaps—but they are also aware that these bodies are very easily decomposed by acids, and by earthy and metallic salts. We must therefore suppose, that the resin soap has been formed, which is exceedingly improbable; and that it has been decomposed either by the free acid or by the salts, which is more improbable still.

Lastly, in paragraph 19, Mr. De R. charges me with an assertion I never made, and excuses himself for errors he may have committed, etc., etc.

In conclusion, I must apologise to you and your readers for being so exceedingly prolix on so trifling a subject. Mr. De Rottermund's reply is made, however, with such assurance, and with such loud-sounding abuse, that I hope you will excuse my retorting somewhat at length. For the abuse I care nothing; and, as far as regards the scientific objections, I think I have answered *them* in the preceding pages. And I will now leave it to your readers to judge my case—declining (should Mr. De Rottermund still remain unsatisfied,) any further communication on the subject, as I have neither leisure nor desire to occupy space in your valuable Journal, which might be devoted to better purposes than the continuation of so useless and futile a discussion.

Toronto, May 7, 1847.

CRITICAL REMARKS ON THE LABOURS OF MR. DE ROTTERMUND, LATE CHEMIST TO THE GEOLOGICAL SURVEY.

BY HENRY CROFT, Prof. of Chemistry, King's College,
Toronto, No. II. *

In my first paper, I elucidated those parts of Mr. De Rottermund's report which relate to the Tuscarora Spring, and it was my intention to have treated the remaining portions in a similar manner; as on first perusing the pamphlet I thought they were fit subjects for criticism. On carefully re-perusing the report, however, I am induced to alter my opinion; for, if I were to attempt to write a full criticism on the work, I should have to make a dozen remarks on each page, and should occupy more space in your valuable journal than the subject merits. There are, however, a few points on which I may be allowed to make a few observations.

In the commencement, Mr. De Rottermund says, "After having analysed the waters of the St. Lawrence and Ottawa;" (this refers to a paper published in the 1st volume of your journal, No. 5, in the 3d paragraph of which you will find the following statement):—"I obtained so small a quantity of residue from a litre of the Ottawa water, that I found it difficult to weigh it with perfect precision, but I estimated it at 1.5 grains." To allow the weighing accurately 1.5 grains to be an operation of difficulty, is rather a curious admission for an analytical chemist; but it appears that although Mr. De R. is unable to weigh with precision 1.5 grains, he is able to ascertain that in the quantity are contained 0.62 grains of sulphate of magnesia, and 0.38 grs., 0.27 grs., and 0.31 grs. of other salts.

As he does not say that these weights were also estimated, we may take this statement as a fair criterion for the credibility of Mr. De R's quantitative analyses.

Mr. De Rottermund classifies mineral waters under the

* British American Journal, July, 1847.

heads of magnesian, iodurated, saline, antimoniferruginous, sulphurous, and gaseous springs—a classification of no value whatever, as many saline springs may contain variable quantities of iodine and magnesia, and a water does not cease to be magnesian if it contains a little iodine.

At page 7, the George's Spring is mentioned. This water has been analysed by Dr. Hall, McGill College, and found to be somewhat different from the Caledonia water; to which, according to Mr. De R., it is precisely similar as regards its compounds. It contains, he says, neither iron nor hydro-sulphuric acid; while in the preceding page he states that the Caledonia water contains the latter substance, and Mr. Chilton had found iron. Mr. De R. occasionally contradicts himself.

In his analysis of the Kingston water, Professor Williamson found both carbonic and hydro-sulphuric acids. Mr. De R. finds neither, and accounts for it by the water of the original spring becoming mixed with that of others—and this, says Mr. De R., has caused the change. What other change than dilution could have taken place does not exactly appear. Moreover he calls into play a leaden pipe and an iron pump, and adds, "by the motion of the pump the gas is separated from the water the temperature of which is increased by the same." This is a beautiful rationale; the gas is separated from the water, and, in so doing, must, of course, produce a diminution of temperature, but, by the friction of the particles of water on each other, and on the pumps, so much heat is generated as to overcome this and to raise the temperature. This is something entirely new, and we hope soon to have a calculation of the probable quantity of heat evolved by the falls of Niagara.

At page 10, Mr. De R. gives us a profound dissertation on the "Ice Spring," (on the road from Hamilton to St. Catharine's) which he says is *nothing*, and he then proceeds to explain this *nothing*. I must candidly confess that the paragraph is utterly beyond my comprehension. I have endeavoured in vain to arrive at, and may, perhaps be mistaken in the meaning, but the only points which Mr. De R. seems to have made out quite clearly, are 1stly., "that ice cannot be formed without water." 2ndly., "That ice is produced by the temperature of water being lowered" 3rdly., "that salts, by

dissolving, produce cold." These are certainly very novel facts, but not of such a nature as to require discussion.

Mr. De R. attempts an explanation on scientific principles, but, as he scarcely mentions what it is he wants to explain, and the whole paragraph is most illogically written, it is quite impossible to ascertain his real meaning.

In the next page he seems inclined to assume that salt may be separated on the surface of the water by evaporation ; that this salt may then dissolve again and cause the water to freeze.

The first question to be answered is,—Does such a phenomenon really exist? and 2ndly., What are all the circumstances attending it?

When this information has been obtained, I have no doubt an explanation may be offered in more intelligible language than that used by Mr. De R.

At page 16, we have a notice of the Petroleum Spring at Gaspé. This, according to Mr. De R., "is composed of creasote, naphtha, petroleum, bitumen, and paraffine." It would be desirable to obtain from Mr. De R. his ideas concerning the characteristics of these bodies, viz., bitumen, naphtha, and petroleum, for, according to most chemists, the latter contains naphtha, and bitumen.

Mr. De R. then gives us (without acknowledgement), a liberal translation from Berzelius, (*Lehrbuch der Chemie*. Bd. viii. s. 465), concerning the petroleum springs found in other countries. Not one has yet been discovered containing creasote, and as Mr. De R. finds antimony, zinc, and chlorine where they do not exist, it is most probable that his creasote belongs to the same class,—a supposition which can be easily decided by experiment.

Lastly, Mr. De R. mentions an oil spring, and says, "*such springs are really valuable to the country*;" a statement which no one would be inclined to deny, if they really did exist.

In concluding, it is scarcely necessary for me to make any remarks on this most singular production. The few extracts and quotations I have made will enable your readers to form their own conclusions concerning it, and I think they will agree with me in considering it a tissue of the most glaring ab-

surdités, the most erroneous statements, and the most trashy reasoning. It cannot but be regretted that such a work should be printed by order of the Legislative Assembly, for when published under such authority, and by the chemist attached to the survey, under the superintendence of so talented a geologist as Mr. Logan, it must necessarily acquire a character for correctness among the mass of the people, to which it cannot, in reality, have the slightest pretension.

Toronto, April 24, 1847.

OBSERVATIONS SUR LA PARTIE CHIMIQUE DU RAPPORT
DE PROGRES POUR L'ANNEE 1847-8. DE L'EXPLORATION
GEOLOGIQUE DU CANADA.

Par E. S. DE ROTTERMUND, Ecr., Yamaska. *

Je viens de recevoir le Rapport de l'Exploration Géologique du Canada, intitulé, "Rapport de progrès pour l'année 1847-8."

Quant à la partie géologique proprement dite, qui décrit avec une grande précision l'état du globe, depuis l'Atlantique jusqu'au Lac Supérieur, et du nord au sud des possessions Britanniques, chaque épaisseur, chaque inclinaison des différentes couches de terre, de roche, etc., etc., je laisse au jugement de ceux, qui ont plus d'expérience scientifique et pratique dans cette partie des travaux de l'exploration ; mais je vais aborder la partie qui a rapport à la chimie, à laquelle je suis obligé particulièrement de porter attention, non-seulement comme simple citoyen, mais comme homme de science qui a fondé le Laboratoire où Mr. Hunt a fait toutes ces grandes analyses. Par conséquent, j'ai dû y faire quelques travaux aussi, et quoique Mr. Hunt paraisse les ignorer complètement, ou plutôt qu'il s'est créé le droit *de se placer dans une si haute position*, qu'il ne devait pas même condescendre à les mentionner, il n'en est pas moins de mon devoir de dire ce que je pense de ce rapport, et d'en démontrer au public toutes les erreurs, surtout que Mr. Hunt a refait plusieurs analyses

* British American Journal, December, 1849.

déjà faites par moi qualitativement. Si je ne me suis pas montré assez courageux et hardi pour publier les analyses quantitatives, c'est que les moyens de ce laboratoire ne me permettaient pas de les faire, sans exposer le chimiste à perdre la confiance dans ses travaux, en donnant au public des chiffres plus qu'erronés, comme je le prouverai plus bas à Mr. Hunt sur sa propre publication.

Que Mr. Hunt soit cité, à tout propos, par les personnes qui ne connaissent pas la science de chimie, c'est leur affaire; la mienne est de remarquer la valeur des travaux qui sont publiés dans le Rapport de progrès pour l'année 1847-8. Depuis le commencement du Rapport du Chimiste jusqu'à la page 144, je ne peux rien en dire, soit en faveur ou contre; c'est la description de son voyage, je laisse au lecteur à juger s'il en est content ou non. J'aurais peur même de le contredire: il devinera pourquoi je veux respecter sans peut-être partager son opinion; mais arrivé à la page 145 je vais suivre jusqu'à la fin du rapport.

Je ne doute pas que Mr. Hunt a pris tous les moyens et précautions pour emporter tous les échantillons, et faire en sorte, comme il le dit lui même, "que les eaux parvinssent sans avoir éprouvé de changement au laboratoire, où leur analyse a occupé la saison d'hiver, et qu'on n'a épargné aucune peine pour rendre les résultats dignes d'être envisagés avec confiance sous tous les rapports. (On verra s'il y a possibilité de croire à ces résultats.) Plus loin Mr. Hunt dit, "C'est une opinion généralement reçue parmi les Philosophes Chimistes, qu'il y a réellement séparation des acides et des bases dans la solution. Par exemple, dans l'eau de la 'source intermittente de Caledonia,' on trouve du chlore et du brôme avec potassium, sodium, calcium, et magnesium, etc. etc." Il faut que nous nous entendions sur ce principe plus antique que la phlogistique; je ne sais pas de quelle espèce ou de quel siècle de Philosophie il veut parler, car je veux démontrer l'impossibilité de cette doctrine. Chaque fois qu'il y a une combinaison de deux corps, il y a une émanation de la chaleur; et par conséquent, chaque fois que deux corps sont mis en présence, s'il y a une augmentation de la chaleur due à cette circonstance, il y a la combinaison. C'est une

loi de Chimie générale ; preuve, si on met quelque soit acide et la base, chacun séparément dans une eau distillée, qui ne contient aucun autre corps, on remarque l'augmentation de la température suivant l'affinité plus ou moins grande, par exemple, que l'on verse l'acide sulfurique sur l'ammoniaque, ou l'ammoniaque sur l'acide sulfurique, la température est presque celle de l'ébullition, et se forme sulfate d'ammoniaque ; prenez le nitrate de potasse en dissolution et sulfate de soude étendu d'eau, et si vous les versez ensemble vous avez l'augmentation de la température. Pourquoi ? c'est qu'il y a une décomposition et la formation d'un autre sel, celui de sulfate de potasse et de nitrate de soude, et non pas de l'azote, du soufre, du potassium, du sodium, de l'oxygène, comme on pourrait le prétendre en se basant sur le principe de Mr. Hunt. La loi est établie, que les acides les plus forts s'unissent avec les bases les plus fortes, et les acides les plus faibles avec les bases les plus faibles ; c'est sur cette loi que la chimie analytique se base dans plusieurs cas. Donc, Mr. Hunt ne peut pas dire avec ses Philosophes, qu'on ne peut pas savoir dans quel état est la combinaison ; mais pour la connaître, il faut avoir des connaissances justes sur tous les principes des lois fondamentales, et sur la nature des corps. Si Mr. Hunt, comme il le dit, a trouvé dans son analyse du chlore et du brome avec le potassium, il doit savoir, comme vrai chimiste, si c'est du chlorure de potassium ou bromure à l'état de combinaison, ou comme simple mélange ; sinon, c'est moins que pardonnable, surtout pour celui qui prétend de se poser comme juge, et qui a aussi la prétension de ne pas faire des erreurs à 1000000.

J'espère donc que nous allons mettre de côté ces Philosophes qui ne peuvent pas se décider sur l'état des combinaisons, et si Mr. Hunt les a suivis, je lui démontrerai dans chacune de ses analyses subséquentes l'erreur de ce principe. Venons aux analyses des eaux minérales ; je suis très content de voir que Mr. Hunt a une balance si délicate, qu'elle est, comme il le dit dans son rapport, *sensible d'un demi-milligramme sur deux cents grammes* ; il me permettra donc de m'en servir, et de refaire avec lui les calculs avec la même précision, en admettant qu'il a bien posé, d'après son propre aveu. Mr. Hunt dit avoir trouvé dans 1,000 parties d'eau,

	Mr. Hunt.	Vérification par calcul.
Chlore,	4.242810	4.218876
Brome,	0.011730	0.011621
Iode,	0.000461	0.000467
Acide sulfurique,	0.002400	0.002425
Soude,	3.726400	3.746354
Potasse,	0.022100	0.022421
Chaux,	0.082880	0.083309
Magnésie,	0.254600	0.254207
Alumine,	0.004400	
Silice,	0.031000	
Fer et Manganèse,	0	
traces,	0	
Acide carbonique,	0.705000	0.705799
	<u>9.063781</u>	

Mr. Hunt déduit de ces ingrédients trouvés, que 1,000 parties d'eau doivent avoir les sels dans les combinaisons suivantes :—

Chlorure sodique,.....	6.967500
“ potassique,.....	0.030940
Bromure sodique,.....	0.015077
Iodure sodique,.....	0.000530
Sulfate potassique,.....	0.005280
Carbonate sodique,.....	0.048570
“ calcique,.....	0.148000
“ magnésique,.....	0.526200
“ ferreux et manganoux, traces,	
Alumine,.....	0.004400
Silice,.....	0.031000
Acide carbonique,.....	0.349000
Eau,.....	<u>991.873503</u>
	1000.000000

Chlorure de sodium est composé de 39.66 de sodium et de 60.34 pour cent de chlore ; donc dans 6.967500 on doit avoir 4.204189.

Chlorure de potassium est composée de 52.53 de potassium, et de 47.47 pour cent de chlore ; donc dans 0.030940 on doit avoir 0.014687 de chlore, lequel ajouté au chlore du chlorure de sodium ne fait que 4.218876.

Mr. Hunt a trouvé 4.242810 ; qu'a-t-il fait avec la différence ? on devrait donc trouver du chlore à l'état gazeux dans

l'eau minérale, selon l'analyse quantitative des corps faite par Mr. Hunt.

Le lecteur voit qu'en examinant chaque item de la composition des sels, donnée par Mr. Hunt, il n'y a pas un seul corps qui a son poids exact, et si on admet que la colonne No. 1 soit véritable, celle No. 2 est erronée, et si le No. 2 est vrai, que peut-on penser du No. 1?

Pour plus grande preuve que les chiffres ne sont pas exacts, c'est qu'en ajoutant à la colonne No. 1, 991.873503, qui est la quantité d'eau qui se trouve dans 1000 parties, on a 1000.-967284.

Je ne crois pas que Mr. Hunt aurait ajouté de l'eau lui-même pour augmenter la quantité. D'où vient-il que Mr. Hunt trouve dans 1000 parties d'eau une quantité de 1000.967284 à part des traces de fer et manganèse.

Un chimiste le plus consciencieux et le plus expérimenté peut faire des pertes, mais jamais trouver plus de ce qu'il a pris pour faire des analyses, comme il dit : *"dignes d'être envisagées avec confiance sous tous les rapports."*

Mr. Hunt ne croirait-il pas que l'augmentation si grande dans ses analyses, lui aiderait à expliquer les grands phénomènes qu'il a cru découvrir en parlant de la silice en dissolution, que ce n'est pas dû à la présence des alcalis dans les eaux minérales, mais à *la grande poussière faite dans son laboratoire.*

On voit donc que les chiffres faits par Mr. Hunt sont des plus erronés, même en admettant que les pesées étaient bien faites, et que chaque corps était bien déterminé. Si la pesée était correcte, on aurait dû dire que dans telle quantité d'après l'évaporation on a eu tant de résidu, et on aurait vu où était l'erreur, tandis qu'avec la méthode que Mr. Hunt a suivie, il y a l'impossibilité, vû qu'on peut ajouter la quantité d'eau nécessaire pour faire paraître les chiffres ronds.

A présent, venons à la nature des corps qui figurent dans les analyses de Mr. Hunt.

L'acide carbonique a la propriété d'éteindre les corps en ignition, tandis que l'hydrogène carboné brûle lui-même. D'où cela vient que Mr. Hunt qui fait des analyses si correctes

qu'il ne fait pas d'erreur à $\frac{1}{1000000}$ un millionième de grain, nous donne la quantité d'acide carbonique à l'état gazeux à $\frac{1}{1000000}$ de grain, et ne donne rien de la quantité immense d'hydrogène carboné qui se trouve dans cette source.

Tous les voyageurs peuvent se rappeler les expériences que le propriétaire faisait en allumant les gaz sortant de la source. Ces gaz d'après l'expérience de Mr. Miller, Professeur de Chimie à Edinbourg, publiée en 1822, prouvent que l'eau absorbe $\frac{1}{30}$ de son volume, et 1 litre de ce gaz pèse 0.721 grammes.

Mr. Hunt croit-il que l'acide carbonique ou l'hydrogène carboné sont la même chose, à cause que ce sont des gaz, ou que cela était plus facile et commode pour son analyse de les mettre de côté ?

La "Source au Soufre Blanc."

" Bien qu'elle porte le nom d'eau sulfureuse, son titre à cette dénomination n'est pas très fondé, elle a une saveur et une odeur faiblement sulfureuse et elle noircit légèrement les sels de plomb et d'argent; mais la quantité de soufre existante, soit comme hydrogène sulfuré, soit comme sulfure alcalin, est très peu considérable et ne peut pas être estimée quantitativement par les procédés ordinaires."

Mr. Hunt lui-même donne le nom de soufre blanc à la source, puis il dit : " Bien qu'elle porte ce nom, ce n'est pas très fondé," et immédiatement il ajoute qu'elle a une saveur et une odeur faiblement sulfureuses et sensible sur les sels d'argent et de plomb, et il ne sait pas si c'est de l'acide sulfurique ou du sulfure alcalin, quand plus bas il dit : " plusieurs bouteilles d'eau furent mêlées, à la source, avec la solution d'arsenic, mais le précipité de sulfure arsénieux fut à peine perceptible : la quantité d'hydrogène sulfuré n'équivalait pas à un ponce cube par gallon." Donc c'est de l'hydrogène sulfuré qu'il croit avoir obtenu, ensuite il ajoute : " Il est néanmoins suffisant pour communiquer à l'eau des propriétés médicinales, car il est bien attesté que l'efficacité de cette source pour le rhumatisme et les affections cutanées surpasse celle de toutes les autres." Je vois que les estomacs des pauvres sont de meil-

leures preuves que ces travaux scientifiques de Mr. Hunt, car dans ses chiffres il ne donne pas même les traces ; il dit " qu'on ne peut pas l'estimer quantitativement par les procédés ordinaires," et " qu'il y a à peu près un ponce cube par gallon d'acide sulphydrique," donc ce n'est pas le sulfure alcalin comme il le supposait.

Voyons donc si je ne pourrais pas trouver la conclusion dans la dénomination des corps de son analyse.—Pas du tout. Cette source a la même combinaison chimique que les autres, et avec les chiffres ronds 1000.

Malgré l'iodure de sodium, le carbonate de fer comme traces, voilà encore la quantité de soufre, soit " comme hydrogène sulfuré, soit comme sulfure alcalin," abandonné dans les analyses quantitatives, mais n'importe, on reste avec les chiffres ronds, 1000. J'espère donc qu'il n'y a pas de nécessité à refaire de pareils calculs, car il est plus que visible que la vérité ne se trouve pas plus dans les chiffres, que dans les descriptions de corps composants.

Mais venons donc à la grande découverte qui s'est faite dans le laboratoire de progrès de 1847-8, qui va éblouir le monde savant et les frapper de stupeur ; mais comme la découverte est bien grave, laissons parler l'auteur lui-même.

" La grande quantité de silice qu'elle contient est une particularité intéressante, et qui se rattache naturellement à la nature fortement alcaline de l'eau. Comme la silice est capable de décomposer une solution de carbonate sodique, il est probable qu'une portion de la soude existe à l'état de silicate. Vu l'incertitude qui règne encore quant à la composition de ces silicates solubles, il est impossible de calculer quelle portion de la soude il faudrait déduire de celle qui est représentée comme existant à l'état de carbonate, mais une expérience indirecte jette quelque jour sur la question. On fit évaporer 1000 grammes de l'eau jusqu'à siccité parfaite, pour rendre toute la magnésie insoluble. Le résidu après avoir été dissous dans de l'eau distillée, fut mêlé avec une solution de chlorure barytique, et donna un précipité de carbonate avec un peu de sulfate, qui contenait une quantité d'acide carbonique correspondant à 2540 de carbonate sodique, tandis que la quantité

de soude en sus de ce qu'il en fallait pour saturer le chlore, le brôme et l'acide sulfurique, égalait 1338 parties de carbonate. La différence 2018 correspond à 1170 de soude pure, ce qui peut être regardé comme formant un silicate avec les 0840 de silice. Vu la connaissance imparfaite que nous avons des silicates, et particulièrement des silicates solubles, il est évidemment inutile de s'étendre davantage sur le mode de combinaison sous lequel ces substances existent.

“ La composition de 1000 parties de l'eau donne :—

“ Chlorure sodique,.....	3.84300
“ potassique,.....	0.02300
Bromure sodique,.....	0.01004
Iodure sodique, traces,.....	
Sulfate sodique,.....	0.01833
Carbonate sodique,.....	0.45580
“ calcique,.....	0.21000
“ magnésique,.....	0.29400
“ ferreux, traces,.....	0.
Alumine,.....	0.00265
Silice,.....	0.08400
Acide carbonique,.....	0.14100
Eau,.....	994.91818
	1000.00000

“ La quantité de matières solides dans 1000 parties de cette eau est de 4.9406 parties, et la composition d'une livre de 7000 grains est comme suit, etc., etc.”

Mr. Hunt dit qu'il a trouvé dans 1000 parties de cette eau 4.9406 de matières solides; plus haut il dit que dans mille parties de cette eau il y a 994.9181, ajoutez ensemble matières solides à cette eau, on a 999.8587. Que le lecteur juge s'il y a de la vérité dans les chiffres. Je crois que de cette découverte l'on ne retirera pas plus d'utilité ni de connaissances, que l'enfant d'une bulle de savon qu'il voit en plusieurs différentes couleurs, mais s'il veut la saisir, il n'a que des goutelettes d'eau pour certitude.

Mr. Hunt dit que la grande quantité de silice que l'eau contient est une particularité intéressante et qui se rattache naturellement à la nature *fortement alcaline de l'eau*. Comment cela? Pour que l'eau soit alcaline, il faut qu'il y ait des al-

calis libres ou des sels basiques, ce qui n'existe pas d'après sa propre analyse. Le chlorure, bromure, iodure, et le sulfate ne sont pas des sels alcalins ; de plus, Mr. Hunt a trouvé l'acide carbonique libre dans cette eau, donc elle ne peut pas être alcaline ; la silice n'est jamais soluble dans aucune eau alcaline ; mais le silicate de soude.

C'est sur ces principes qu'est fondée l'analyse des silicates. Car, si c'est comme Mr. Hunt voudrait prétendre, on aurait pu dissoudre les grenats, cristaux de roche, rubis et divers autres minéraux dans l'eau alcaline, tandis qu'il faut les mettre au feu, avec cinq à dix fois leur poids de carbonate de soude.

Dans son analyse quantitative il a eu seulement *la silice pure*, et il n'a pas de soude pour former le silicate alcalin pour les rendre solubles. Mr. Hunt trouve dans toutes les eaux sans distinction, la silice pure avec l'acide carbonique, et jamais la soude pour rendre l'eau alcaline. Il dit : " comme la silice est capable de décomposer une solution de carbonate sodique, il est probable qu'une portion de la soude existe à l'état de silicate."

C'est bien amusant ceci. Il est vrai que le proverbe dit " Toute vérité n'est pas bonne à dire," mais en chimie il n'y a pas de proverbes—mais des lois. Aussi l'on dit : " La silice décompose le carbonate de soude *au feu*," car les carbonates sont décomposables au feu, et la silice jouant le rôle d'un acide qui est stable, forme le silicate plus ou moins soluble suivant la nature de la base, lequel silicate de nouveau se décompose en présence de l'acide carbonique.

Allons plus loin, il dit que : " Vu l'incertitude qui règne encore, etc., etc."

Mr. Hunt aurait mieux fait de dire, vû que je ne sais pas, *moi personnellement*, quelle est la nature des silicates ni pourquoi la silice est en dissolution dans les eaux que j'analyse, ni comment m'y prendre pour résoudre cet accident, quoique j'aie tant évaporé, j'aie tant filtré inutilement, que j'aie rendu tous mes ballons inserviables, tellement je les ai attaqués par les acides qu'ils sont devenus presque mats, et usés à moitié : si quelqu'un peut me dire, je lui serai bien reconnaissant, car par ordre du directeur de l'Exploration Géologique, je dois refaire

les analyses de mon prédécesseur, qui n'a pas voulu se soumettre à écrire des chiffres : en attendant je vais mettre cela sur le dos de la science et de mes *philosophes*, "qu'il y a réellement séparation des acides et des bases, dans la solution : " quant à moi, je ferai mes chiffres ronds de 1000, et que le Gouvernement s'arrange comme il le voudra. A cela j'aurais donné le conseil d'un chimiste, *chauffez moins, filtrez plus rarement, surtout quand il n'y a pas de besoin, et ne vous servez pas de verres qui ont des silicates de soude, que les acides dissolvent, et par là augmentent les analyses*, (comme les chiffres de Mr. Hunt en sont un exemple.) *Mettez vos travaux à l'abri de la poussière du Laboratoire* ; ensuite tachez de comprendre qu'il y a une différence marquante entre le soufre de l'acide sulphydrique décomposé à l'air, et la matière organique qu'on appelle *barégine*, qui flotte aussi, et qui se trouve dans cette eau, comme entre les corps selenhydriques et les choux pourris, malgré que tous les deux ont les mêmes arômes ; ce qui prouve que le chimiste peut utiliser son nez, mais non pas juger seulement par le nez.

La "Source sûre" de Tuscarora.

C'est la source à laquelle j'ai donné le nom Antimoni-ferreuse, à cause de la présence d'antimoine trouvé par moi. C'est avec la plus grande peine que je me décide à faire les remarques suivantes sur l'analyse de Mr. Hunt, mais j'y suis forcé par la nature de sa publication. Je donne la copie textuelle de l'examen de l'eau par Mr. Hunt comme preuve de ses connaissances en chimie, afin que le lecteur puisse juger si la personne est capable d'autre chose que peser de l'eau, et faire aboutir ses comptes en chiffres ronds de 1000, au lieu de corriger les travaux d'un chimiste qui est reconnu comme tel par les premiers chimistes de l'Académie des Sciences de Paris.

Examen de l'eau par Mr. Hunt.

" La pesanteur spécifique s'est trouvée de 1005.583. Une solution de nitrate argentique ne l'affecta pas sensiblement, montrant l'absence de chlore, mais des sels barytiques solubles produisirent d'un coup un précipité copieux, insoluble dans un acide quelconque, indiquant que l'acide présent dans l'eau était

le sulfurique." Mr. Hunt dit qu'il n'a pas eu de précipité avec le nitrate d'argent; il est plus qu'en erreur; car s'il n'en avait pas eu, il n'aurait pas dit: "une solution de nitrate d'argent ne l'affecta pas sensiblement," c'est-à-dire, qu'il y avait toujours pour les chimistes, quoique non pour les droguistes. Sulfate d'argent est aussi insoluble, donc il a dû y avoir un précipité; en se servant du nitrate argentique le premier au lieu de sels de baryte, il a confondu le chlore avec l'acide sulfurique. Plus tard il dit qu'il a eu du chlorure alcalin, comment cela? on ne voit pas qu'il l'ait converti en chlorure par aucun procédé direct, si ce n'est qu'il s'est servi du chlorure barytique; mais comment peut-il être certain que tout son alcali aurait pu être converti en chlorure, s'il ne s'est pas servi de l'acide chlorhydrique; certes, ce n'est pas le moyen sûr et correct d'avoir du chlorure pour doser, surtout pour un savant chimiste qui prétend ne pas se tromper pas même à un billionième de gramme.

"Les réactifs ordinaires appliqués à l'eau récente montrèrent que le fer était dans un état de proto-sel, condition nécessairement liée, en effet, à la présence de l'hydrogène sulfuré. Concentré par l'évaporation et mêlé d'un peu d'acide nitrique, l'ammoniaque donna un copieux précipité brun-rouge: une portion de matière organique dans le liquide empêcha la parfaite précipitation du fer, et le sulphydrate ammoniaque fut en conséquence ajouté en même temps. Le précipité ainsi obtenu, après avoir été complètement lavé, fut dissous dans de l'acide hydro-chlorique, puis bouilli avec de l'acide nitrique, filtré et précipité par l'ammoniaque avec l'addition préalable de sel ammoniaque, et encore filtré. La transparence du liquide filtré, ainsi obtenu, ne fut pas troublée par le sulphydrate d'ammoniaque, circonstance indiquant l'absence du manganèse et autres métaux de cette classe, y compris le zinc, le nickel et le cobalt. Le précipité se trouva en partie soluble dans une solution de potasse; la portion soluble était l'alumine, et le résidu le peroxyde ferrique avec un peu de magnésie."

Eh bien! que Mr. Hunt montre donc cela à un chimiste, mais à un véritable chimiste, non pas à ceux qui sachant un

peu filtrer, évaporer, et quelques noms propres de réactifs, portent de suite le nom de chimistes, (quelques fois même de minéralogistes, astronomes,) et il verra si, après avoir lu son rapport, il osera donner aucune valeur quelconque à ses analyses. Comment ! il a eu d'après lui-même un précipité copieux par l'ammoniaque avant de se servir de sulphydrate d'ammoniaque, et il a pris tout cela seulement pour du peroxyde de fer. Puis quand il a obtenu ce précipité copieux qu'il a (à ce qu'il dit) traité par le sulphydrate d'ammoniaque sans mentionner si ce sulphydrate d'ammoniaque a produit un précipité ou non, il reprend ce précipité pour dissoudre dans l'acide hydrochlorique, puis il le fait bouillir dans l'acide nitrique, encore il le filtre, il précipite, par l'ammoniaque, il filtre encore et ne dit que ce qu'il a obtenu par ces deux filtrations, seulement il dit que la transparence du liquide filtré n'était pas troublée par le sulphydrate d'ammoniaque, ensuite il ajoute que le précipité se trouva en partie soluble dans la potasse, sans dire quel précipité. Est-ce le précipité obtenu par l'ammoniaque, ou celui de sulphydrate d'ammoniaque, ou celui qu'il a dû obtenir sur le filtre après avoir dissous dans l'acide chlorhydrique et ensuite bouilli avec de l'acide nitrique, ou du second filtre quand il a traité la dissolution de l'acide chlorhydrique par l'ammoniaque ? Il dit seulement que le précipité traité par la potasse lui a donné pour le résidu le peroxyde de fer avec la magnésie et l'acide phosphorique en dissolution avec l'alumine, sans dire par quel moyen il s'est assuré que tous ces précipités abandonnés sur les filtres ne contenaient pas d'autres matières. De grâce, que Mr. Hunt ouvre donc les ouvrages élémentaires, qui lui en diront assez sans que je sois forcé de lui citer un traité complet de chimie. Plus bas, il dit :

“ L'arsenic, l'antimoine, le plomb, l'étain et le cuivre ont tous été découverts récemment dans différentes eaux ferrugineuses de l'Europe ; mais la présence de l'hydrogène sulfureux libre dans l'eau récente de la présente source est incompatible avec leur existence en solution.”

Le Professeur Croft, dans une polémique par rapport à cette source, m'a traité avec de beaux sarcasmes, me désignant comme un ignorant complet dans la science de chimie, à cause

que j'avais osé dire qu'un sel d'antimoine se trouve en dissolution dans une eau minérale ; il citait à l'appui de son jugement tous les ouvrages qu'il avait dans la collection de sa bibliothèque, qui ne disaient rien sur l'existence de ce corps dans les eaux minérales, et par là il a voulu prouver qu'il est impossible que les sels d'antimoine puisse exister dans l'eau minérale excepté (avec ?) la chaux, la potasse, la magnésie, etc., et **DE L'EAU EN ABONDANCE** ; tandis que Mr. Hunt paraissant ou voulant être plus à la hauteur des nouveautés scientifiques de l'époque, dit que l'antimoine a déjà été trouvé en Europe, niant son existence dans celle de Tuscarora.

Mr. Hunt voudrait-il me dire quand, et par qui l'existence de l'antimoine dans l'eau a été trouvée, et il verra que j'ai été le premier qui a fait cette importante découverte pour la science de chimie et de médecine ; mais je lui dirai que pour le trouver quoiqu'il est plus facile quand on est prévenu, qu'il n'est pas capable même de constater ; car la marche qu'il a suivie et qu'il décrit parle par elle-même. Mr. Hunt, dit que l'hydrogène sulfureux libre dans l'eau récente de la présente source est incompatible avec leur existence en solution, et plus bas il ajoute :—" la quantité d'hydrogène sulfuré présente est petite, ne faisant qu'environ un demi-pouce cube dans 200 pouces cubes d'eau ;" donc il y avait de l'acide sulphydrique ; mais en présence de protoxyde de fer, l'acide sulphydrique est aussi incompatible, car l'hydrogène sulfureux ou l'acide sulphydrique précipite le protoxyde de fer. Soyez donc compatible avec vos idées et vos travaux, Mr. Hunt. Quant il dit qu'il a trouvé dans la même source le peroxyde de fer, et le protoxyde et de plus qu'il a dosé le tout à l'état de sulfate ferreux, quoiqu'il disait plus haut qu'il a trouvé le protoxyde par l'acide sulphydrique, et le peroxyde par l'ammoniaque, le sulfate d'ammoniaque et la potasse. Est-ce que par hasard quelques philosophes lui ont appris que l'oxide ferrique qui a la composition, FeO_2 est la même chose que ferreux qui a la composition, FeO , que quand on trouve tous les deux, on les prend seulement comme si c'était sulfate ferreux pour avoir la facilité de mettre un chiffre rond de 1000.

Comment, Mr. le chimiste peut-il faire des analyses quali-

tatives, sans faire attention aux matières organiques et plus encore dans les analyses quantitatives?

Mr. Hunt ne sait-il pas que plusieurs matières minérales en présence des matières organiques ne peuvent pas être découvertes par aucun réactif chimique? Mr. Hunt dit qu'il a trouvé de l'acide phosphorique; je suis fâché qu'il n'ait pas voulu donner la description par quel réactif et dans quelle période de l'analyse il l'a remarqué, car je crains que la présence de l'acide phosphorique ressemble à l'histoire de la silice. Que Mr. Hunt remarque encore que dans la correction qu'il a faite à l'analyse de Mr. Croft, qui a trouvé le fer à l'état du peroxyde, et lui, tantôt à l'état du peroxyde, tantôt comme protoxide, et tantôt il donne la quantité du peroxyde de fer avec l'alumine, quand ils sont en si grande abondance tous les deux. C'est un peu amusant de voir des escapades de cette nature faites par un chimiste qui ne se trompe pas à un millionième de gramme, et qui corrige les travaux des autres avec un air si assuré.

A présent, examinons les chiffres. Qu'est-ce que cela signifie que Mr. Hunt trouve dans la même source la quantité d'acide sulfurique avec la formule S O_3 et ensuite S H O_4 ? Je crois qu'il y a là lieu à l'incompatibilité, même avec la quantité suffisante de matières organiques; mais, cela à part, d'où vient que Mr. Hunt dit avoir trouvé acide sulfurique avec la formule S O_3 la quantité 4.63500, et ensuite, représentant les bases comme combinées avec leur équivalent d'acide sulfurique, nous avons pour la composition dans 1000 parties de l'eau, acide sulfurique libre avec la formule S H O_4 la quantité 4.28952. Aurait-il la prétention de trouver un phénomène dans son laboratoire ou l'acide sulfurique libre de Nordhausen qui a la formule S H O_4 en présence d'une quantité immense d'eau aurait pu être toujours à un atôme d'eau. C'est peut-être depuis l'époque de la tempérance que l'acide sulfurique aurait dédaigné de se combiner avec de l'eau, préférant de l'alcool.

Mais, Mr. Hunt a bien réussi dans son analyse, vû, qu'en ajoutant 993.83837 parties d'eau, à part de l'acide phosphorique et une très grande quantité de matières organiques, qui l'ont bien embarrassé d'après son propre aveu, il a eu son

chiffre rond de 1000, et le rapport paraît plus correct, plus volumineux, plein de chiffres, les uns au-dessus des autres, ce qui devrait plaire, surtout à ceux qui aiment à en imposer avec des chiffres, afin de ne pas être pris pour des visionnaires, mais pour des gens de science pratique.

St. Césaire, Novembre, 1849.

REPONSE AUX OBSERVATIONS DE E. S. DE ROTTERMUND, ECR.,

SUR LA PARTIE CHIMIQUE DU RAPPORT DE PROGRES POUR
L'ANNEE 1847-8, DE L'EXPLORATION GEOLOGIQUE
DU CANADA.

Par T. S. HUNT, Chimiste à l'Exploration Géologique.*

Le numéro de décembre, de ce journal, contient un article donné pour être une critique faite par M. E. S. de Rottermund, de mes labours comme Chimiste et Minéralogiste de l'exploration Géologique de la province, tels qu'exposés dans le Rapport de Progrès pour l'année 1847-48.

Que les travaux d'un homme public soient exposés à la critique, il n'y a là rien d'étrange ; mais ce doit être un sujet de surprise, dans le cas présent, que le rôle d'Aristarque soit joué par un individu qui, comme le prouve clairement l'article sous considération, ignore presque entièrement les premiers principes de la chimie.

Je me permettais donc, afin de complaire au désir de quelques-uns de mes amis, d'appeler pour un moment, l'attention des savans sur l'absurdité des accusations portées contre moi, non pas tant pour ma défense personnelle, car je puis à peine concevoir qu'une *pareille* attaque puisse faire tort à ma réputation scientifique, que par devoir envers le public, dont je suis le serviteur, et qu'on ne peut guère supposer en état de distinguer le vrai du faux, dans un écrit dont l'auteur fait ses assertions d'un ton si magistral.

(1) M. de Rottermund fait allusion aux recherches qu'il a faites dans le Laboratoire de l'Exploration, durant les deux années qu'il a occupé le poste de chimiste, et suggère que je

* From the British American Journal, January, 1850. p. 230.

les ignore, ou que je n'ai pas voulu condescendre à en faire mention. Je regrette qu'il n'ait pas été capable d'apprécier le motif de mon silence. Le document non-officiel donné pour être son Rapport, comme assistant de l'Exploration pour le département chimique, a déjà été examiné dans ce journal, par le Professeur Croft de Toronto, dans une critique dont la justice, quant à la science, est manifeste à tous ceux qui sont en état d'en juger, et il m'a semblé qu'après une exposition si complète de ses erreurs et de ses absurdités, toute allusion de ma part, n'aurait été que le souvenir inofficieux de sa malheureuse controverse et de sa défaite.*

(2) A l'égard de mes remarques, page 139, sur la répartition des acides et des bases dans une solution, comme une opinion généralement reçue parmi les philosophes chimistes, M. de Rottermund observe : " Il faut que nous nous entendions sur ce principe plus antique que le phlogistique. Je ne sais pas de quelle espèce ou de quel siècle de philosophie (il?) veut parler, car je veux démontrer l'impossibilité de cette doctrine." Et ceci est suivi d'une démonstration qui prouve clairement que M. de Rottermund est absolument incapable de comprendre le langage commun. Mais je ne veux pas que ce qu'il affirme de l'antiquité de cette doctrine induise ses lecteurs à croire que les découvertes des temps modernes ne l'ont pas confirmée, et qu'elle n'est pas admise par les philosophes chimistes de la présente époque. Je citerai ce que dit Sir Robert Kane, dans ses *Elémens de Chimie*, publiés en 1842.

" Si les acides et les bases ne diffèrent pas grandement en énergie d'affinité, ils s'arrangent de manière à ce que chaque base soit partagée entre tous les acides, et chaque acide entre toutes les bases, en quantités qui dépendent des quantités de chaque acide et de chaque base qui peuvent être présentes, et de sa force d'affinité." Edit. Am. p. 168.

On ne s'exprime pas autrement dans la dernière édition des *Elémens* du Dr. Turner, rédigée par Justus Liebig, de l'Université de Giessen, et publiée en 1842.

" Quand deux acides et deux bases se rencontrent ensemble en proportions neutralisantes, on en doit inférer que chacun

* Mars 1847, p. 289 ; aussi Mai 1847, p. 10, et Juin, p. 36.

des acides s'unit avec les deux bases, d'après un mode réglé par leurs forces respectives d'affinité, et que quatre sels se trouvent contenus dans la solution. De même, la présence de trois acides et de trois bases donnera naissance à neuf sels, et quand quatre de l'un et de l'autre sont présents, seize sels seront produits. Cette manière de voir offre la théorie la plus plausible de la constitution des eaux minérales et des produits qu'elles donnent par l'évaporation." p. 148.

(3) Le critique paraît disposé à s'égayer aux dépens de ma balance, dont la délicatesse le frappe, comme étant extraordinaire ; l'instrument fait certainement honneur à Deleuil même, dont les balances ont à Paris la réputation de ne pouvoir pas être surpassées en justesse ; mais la manière dont il s'en sert peut donner lieu à une plus grande hilarité. Quiconque est au fait des analyses quantitatives, sait que les chimistes ont pour habitude d'exposer sous une certaine forme, qui est de convention, les quantités respectives des différentes substances trouvées dans l'eau : ainsi, par exemple, les sels de sodium dans une eau minérale, soit comme carbonate, sulfate, chlorure, bromure ou iodure, sont changés en un composé d'une constitution définie et connue, comme chlorure sodique ou sulfate sodique ; et c'est d'après la quantité de ce composé que la quantité de la soude est calculée.* La quantité du chlore et des autres radicaux combinés avec le sodium est alors déterminée, et comme le chlore se combine directement avec le sodium, l'équivalent d'oxygène qui est représenté comme combiné dans la soude est soustrait de la somme des poids du chlore et de la soude, pour donner le montant du chlorure de sodium. Les autres calculs sont faits de la même manière, et les proportions dans lesquelles toutes ces combinaisons sont effectuées, sont déterminées par les nombres équivalents, qui, dans le fait, sont les proportions relatives de combinaison de différentes substances. Ces nombres ne sont encore déterminés qu'approximativement ; mais les raffinements dans la manipulation chimique, nous mettent, d'année en année, en état de corriger les déterminations précédentes, et d'offrir un calcul plus approximatif.

* Voir Fresenius. *Analyse Quant.* p. 489, et *Annalen der Chemie und Pharmacie*, t. iii, p. 66.

La composition des différentes combinaisons données dans 1000 parties d'eau, a été calculé d'après les nombres corrigés récemment et posés par Fresenius, ci-devant de l'Université de Giessen, maintenant de Wiesbaden, dans son *Traité*, publié en 1846. Sous ces circonstances, M. de R. en vient à examiner l'exactitude de mes résultats, et prenant non pas mes déterminations originales au moyen de la balance, mais les calculs faits d'après ces déterminations, de la manière qui vient d'être décrite, il entreprend de faire la preuve de ces calculs, mais malheureusement, il a recours, non aux nombres de 1846, mais à ceux donnés par Rose, dans la première édition de son *Traité Pratique d'Analyse Chimique*, sous la date de 1832 (qui sont ceux qu'il cite,) et donne les résultats ainsi obtenus comme servant à corriger les miens !

Il ajoute de plus, "qu'en examinant chaque item de la composition des sels, donné par M. Hunt, il n'y a pas un seul corps qui a son poids exact." Ils doivent comme de raison, être différents de ceux qu'il a déduits des anciens nombres atomiques, mais ils sont aussi exacts que possible, puisqu'ils sont calculés en stricte conformité aux tables corrigées des proportions de combinaisons. La remarque qu'il fait, que suivant mon analyse, "on devrait trouver du chlore à l'état gazeux dans l'eau minérale," est une illusion provenant de la même erreur dans ses chiffres, bien qu'il puisse être démontré qu'un excès de soude comme carbonate, ou carbonate et silicate, ôte même pour M. de R., la nécessité d'une telle supposition.

Mais les remarques sur cette analyse sont terminées par une bévée bien capable d'exciter l'admiration ; pour fournir une preuve encore plus convainquante de l'inexactitude de mes résultats, il a additionné les quantités, données conformément à l'usage ordinaire, de soude, de chaux, de magnésie, de chlore, d'acide sulfurique, etc., et ayant ajouté à la somme la quantité d'eau donnée comme résidu en 1000 parties, il trouve, à sa grande surprise, 1000.967824 parties. Il semble ignorer que la soude et le chlore éliminent, en se combinant, 8 parties d'oxygène sur 58.5 parties du chlorure de sodium formé, et que c'est là la différence, quand au poids, entre le chlorure de sodium, qui existe dans les eaux minérales, et la combinaison incon-

nue du chlore avec l'oxyde de sodium, sur laquelle M. de Rotterdam base sa *correction* ! " L'augmentation si grande" dans mes analyses n'a donc d'existence que dans ses ridicules méprises. Pour ce qu'il est de la quantité d'eau ajoutée " pour faire paraître les chiffres ronds," j'ai seulement à dire que, quant au plan de donner la composition de 1000 parties d'eau, j'ai eu pour l'adopter, l'autorité du Dr. Schweitzer, dont les analyses d'eaux minérales sont connues de tous les chimistes. L'analyse de l'eau de mer du Pas-de-Calais, citée par moi, p. 161 du rapport, en est un exemple.

Pour ce qui est des gaz, M. de Rotterdam aurait pu s'épargner la question, " M. Hunt croit-il que l'acide carbonique ou (et ?) l'hydrogène carboné sont la même chose, etc., ?" s'il avait lu la description du procédé adopté pour l'exacte détermination du gaz acide carbonique à la source, lequel, étant fondé sur le fait qu'il est de la nature de ce gaz de former un sel avec la chaux, ne permet pas qu'il soit confondu avec les gaz adventices, l'oxygène, le nitrogène, et l'hydrogène carburé, qui sont présents dans ces eaux en quantités peu considérables et variables.

(4) Mais pour passer à la " Source au Soufre Blanc," dont je remarque que " bien qu'elle porte le nom d'eau sulfureuse, son titre à cette dénomination n'est pas très fondé ; elle a une saveur et une odeur faiblement sulfureuse, et elle noircit légèrement les sels de plomb et d'argent ; mais la quantité de soufre existante soit comme hydrogène sulfuré, soit comme sulfure alcalin, est très peu considérable, et ne peut pas être estimée quantitativement par les procédés ordinaires." Bien que je donne comme *vulgaire* le nom par lequel cette source est généralement désignée, M. de R. dit : " M. Hunt lui-même donne le nom de Soufre Blanc à la source," et puis il s'efforce de faire douter de l'exactitude de mon exposé, quant à la petite proportion de soufre qu'elle contient. J'ai parlé dubitativement quant à la condition du soufre, parce qu'il est admis par les écrivains les plus récents, que l'exactitude, ou la justesse des procédés proposés jusqu'à présent pour déterminer, est très-douteuse,* et quand la quantité est petite au point d'être à peine appréciable, ce ne pourra être que par induction, comme

* Fresenius Anal. Quan. p. 487.

en sera convaincu quiconque est au fait du procédé, qu'on pourra déterminer si l'ingrédient sulfureux est un sulfure alcalin ou le gaz hydrosulfurique. J'ai fait allusion, dans mon rapport, au témoignage de médecins, quant à l'efficacité de cette eau, dans les cas où l'on suppose que le soufre est efficace, et le critique trouve, ou feint de trouver, quelque chose dérisible dans l'idée que d'aussi petites doses de soufre puissent produire des effets sensibles. Contredira-t-il le savant Dr. Daubeny, professeur de chimie, d'Oxford, et la profession médicale généralement, sur l'efficacité qu'ils attribuent à l'iode, comme constatée dans les eaux de Spa, telle que celle d'Adelheidsquelle, qui, d'après l'analyse de Struvé, ne contient que $45 \frac{1}{114}$ de cet élément, ou dans l'eau de Saratoga dite de Congress, qui a moins de $144 \frac{1}{666}$ d'iode, et environ $48 \frac{1}{666}$ de brôme ?

Pour ce qui est de la présence de la silice dans les eaux telle que considérée, p. 146, il en parle (en s'efforçant de prendre le ton railleur et ironique,) comme de "la grande découverte qui s'est faite dans le laboratoire, qui va éblouir le monde savant, et les frapper de stupeur--mais comme la découverte est bien grave," &c. En ai-je parlé comme d'une nouvelle découverte ? non, car je savais que c'était un ingrédient qui ne manquait jamais dans les eaux naturelles, et que sa présence a été reconnue par Berzelius, par Struvé et par Schweitzer. J'ai suggéré qu'elle pouvait exister dans les eaux alcalines comme silicate, et depuis que mon rapport a été écrit, je trouve que M. Henry, dans ses examens des eaux minérales des Pyrénées† en est déjà arrivé à la même conclusion, et dans son analyse de la source de Chatenois, il a ("vu l'incertitude qui règne encore," quant à la composition des silicates solubles,) représenté la soude comme un mélange de carbonate et de silicate, dont il n'entreprend pas de déterminer les proportions relatives. ‡ Telle est la découverte dont M. de R. me fait erronément honneur.

Mais le sage et savant critique est grandement en peine de savoir comment l'eau en question peut être alcaline, et il parle

† Journal de Chimie et de Pharmacie, tom. vii. p. 15.

‡ Bul. de l'Académie de Médecine 1844-5, p. 160.

d'alcalis et de sels basiques, sans pouvoir trouver la solution de la difficulté qui l'embarrasse. S'il avait été à la cuisine ou à la buanderie, sans recourir à de plus hautes autorités, le cuisinier ou la blanchisseuse auraient dû lui apprendre que le *carbonate de soude* que l'eau contient, la rend alcaline. Mais il ne peut comprendre comment le carbonate de soude peut rendre la silice soluble ; il est vrai, comme il le dit, que la silice décompose le carbonate de soude *au feu* ; mais c'est aussi un fait qu'il aurait pu apprendre dans un ouvrage élémentaire quelconque, que la silice est facilement soluble dans une solution bouillante de carbonate de soude, propriété que le chimiste analytique appelle très souvent à son aide ; et c'est aussi un fait qui ne devrait pas être ignoré de mon prédécesseur, que quand une solution diluée de silice, ainsi obtenue, est neutralisée par un acide quelconque, la totalité de la silice demeure en solution, en ce que Berzelius a décrit comme "la modification soluble," soluble tant dans l'eau pure que dans les acides.* Il prend sur lui de parler de la méthode d'analyser les minéraux silicieux, mais tous les résultats qu'il obtiendra, sans tenir compte de ces réactions, seront bien éloignés de la vérité.

En voulant corriger mes chiffres, dans l'analyse de la "Source au Soufre blanc," il est tombé dans une étrange méprise, dont il est aisé de s'apercevoir sans être chimiste. La table montre les divers ingrédients salins avec l'acide carbonique, en sus de ce qui est nécessaire pour former des carbonates, et le reste est l'eau qui complète les mille parties. J'ai donné au-dessous de cette table, la somme de toutes les matières solides : cette somme, le savant critique l'ajoute à celle de l'eau, *en oubliant l'acide carbonique*, et trouve, comme de raison, que la somme est 1000 parties, moins le montant de l'acide carbonique ; mais, ne s'apercevant pas de sa bévue, il s'écrie : "Que le lecteur juge s'il y a de la vérité dans les chiffres!"

(5) M. de Rottermund en vient ensuite à l'examen des analyses que j'ai faites de la Source Sûre de Tuscarora, et trouve, tout d'abord, une difficulté dans ce que je dis, que l'eau, non seulement ne donne *pas de précipité* avec une solution de nitrate

Voyez aussi Graham's Chem, p. 315, et Rose, Traité Pratique, tom. ii. p. 227.



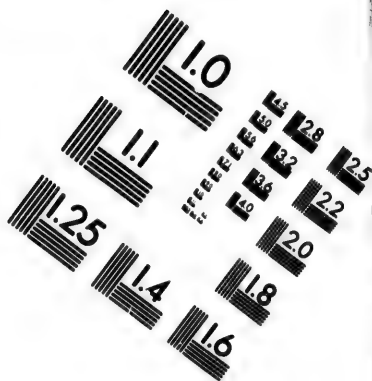
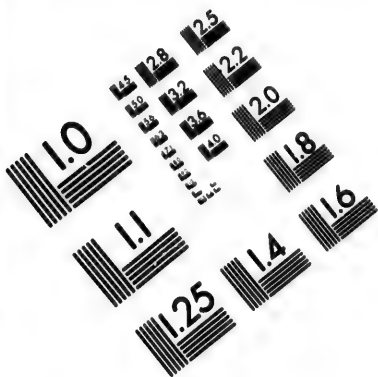
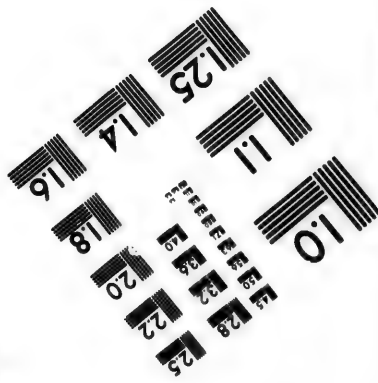
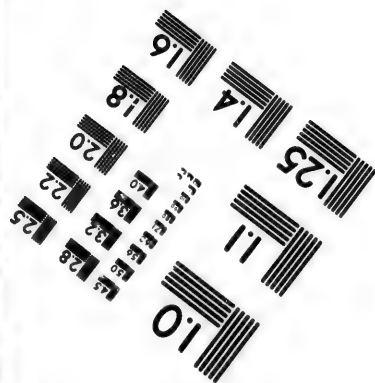
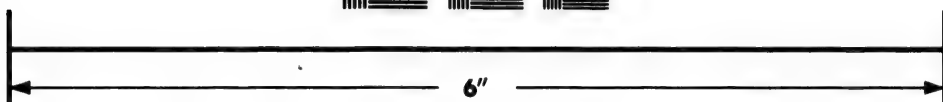
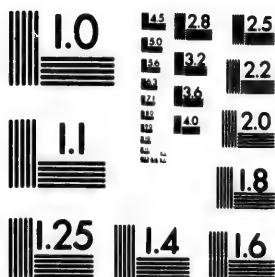


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d'argent, mais que même elle n'en est pas *affectée sensiblement*, c'est-à-dire qu'il n'y a aucun changement visible. Il dit, " (le ?) sulfate d'argent est aussi insoluble ; donc il a dû y avoir un précipité ; en se servant du nitrate argentique le premier, au lieu de sel de baryte, il a confondu le chlore avec l'acide sulfurique." Il n'a qu'à ouvrir un ouvrage élémentaire quelconque, pour apprendre que le sulfate d'argent est soluble dans 88 parties d'eau bouillante, et à un degré très considérable dans l'eau froide, et il pourra se convaincre aisément par l'expérience, qu'une eau contenant une beaucoup plus grande proportion d'acide sulfurique ou de sulfate soluble que la source de Tuscarora, ne donnera pas de précipité par l'addition d'une solution de nitrate d'argent. Il apprendra de plus que la méthode même qui est recommandée par Rose et Fresenius, pour séparer le chlore de l'acide sulfurique, est basée sur la solubilité du sulfate, et l'insolubilité du chlorure d'argent.*

Quant à ses commentaires sur le cours de l'analyse quantitative, j'ai seulement à observer qu'il a eu lieu exactement d'après la routine proposée par Rose et Fresenius, dans des cas semblables,† et que l'omission d'une seule filtration ou précipitation aurait rendu le résultat incomplet et indigne de confiance. Les difficultés qu'il y trouve ne témoignent que d'une ignorance déplorable des notions les plus élémentaires dans la chimie analytique.

(6) Venons en maintenant à la prétendue découverte faite par M. de Rottermund, de l'existence de l'antimoine dans cette source. Ce métal a été découvert, depuis son annonce, avec l'arsenic, l'étain, le plomb et le cuivre, dans quelques sources ferrugineuses d'Allemagne, et il dit, " M. Hunt voudrait-il me dire quand et par qui l'existence de l'antimoine dans l'eau a été trouvée, et il verra que j'ai été le premier qui a fait cette importante découverte pour la science de la chimie et de la médecine ; mais je lui dirai que pour le trouver, quoiqu'il est (soit ?) plus facile quand on est prévenu, qu'il n'est pas capable même de le constater ; car la marche qu'il a suivie et qu'il décrit parle par elle-même."

D'après le ton de ce paragraphe, le lecteur qui n'en saurait

* Fresenius anal. Quant. p. 352.

† Rose, Traité, tom. I. 432, et seq. ; et Fresen. Anal. Qual. p. 223.

pas d'avantage, pourrait supposer que M. de Rottermund a été reconnu par les autorités scientifiques de l'Europe, comme l'auteur de cette découverte. Mais tel n'est pas le cas ; la première annonce de ce fait a été faite à l'Académie des Sciences de Paris, à la fin de l'année 1849, par M. Walchner.† M. Trepier avait, avant cette époque, trouvé de l'Arsenic dans une eau minérale apportée d'Algérie, et M. Walchner réussit à le trouver dans des sources ferrugineuses d'Allemagne, associé avec du cuivre, du plomb, et de l'étain, et dans la source de Weisbaden avec l'antimoine. Cette intéressante découverte a été peu de temps après, confirmée par M. Will, qui a trouvé tous ces cinq métaux dans les sources ferrugineuses de Rippoldsau.§ Selon lui, 10,000,000 parties des trois sources contiennent respectivement 0·16, 0·10, et 0·24 d'une partie d'oxyde d'antimoine. Si M. de Rottermund est, comme il s'en vante, reconnu comme chimiste par les premiers chimistes de l'Académie des Sciences de Paris, comment se fait-il que sa prétendue découverte ne soit pas reconnue par ce corps, et que l'honneur en soit attribué à un autre ? M. de Rottermund affirme que je ne suis pas capable d'en découvrir la présence, "comme le démontre la marche que j'ai suivie." A-t-il lu mon rapport ? Je n'ai suivi aucune marche particulière, ni décrit aucun procédé pour cette fin. Suivant Rose et Fresenius,|| le seul réactif sur lequel on puisse compter pour la séparation complète de l'antimoine d'avec toutes ses solutions, est l'hydrogène sulfuré ou l'acide hydrosulfurique, qui précipite un sulfure orangé. Mais l'eau minérale en question contient déjà de l'hydrogène sulfuré, comme je l'ai fait voir au moyen des réactifs ordinaires ; de sorte que le seul procédé auquel on pouvait recourir pour la séparation du métal, avait déjà été employé dans le laboratoire de la nature. Le fait que la présence de l'hydrogène sulfuré est incompatible avec l'existence de l'antimoine en solution embarrasse M. de Rottermund, mais il essaye de se tirer d'embarras en s'efforçant de démontrer que l'hydrogène sulfuré ne peut pas être présent, en autant que, suivant lui, il est incompatible avec le protoxyde de fer, "car

† Comptes rendus de l'Académie des Sciences xxiii, p. 612.

§ Annalen der Chem. und Pharm. tom. lxi, p. 192.

|| Rose, Traité, tom. ii, p. 214 ; et Fresen. Anal. Quant. p. 230.

l'hydrogène sulfuré, ou l'acide sulfhydrique précipite le protoxyde de fer." Quant à la vérité de cette assertion, tout commençant sait que les solutions de protoxyde de fer ne sont nullement précipitées par l'hydrogène sulfuré. Pour me servir des paroles de Rose, l'hydrogène sulfuré "ne fait pas naître de précipité dans les dissolutions ferreuses neutres.*" La raison en est que la plus faible trace d'acide peut dissoudre immédiatement le sulfure de fer précipité, et l'on se prévaut de cette propriété dans le cours ordinaire de l'analyse, pour séparer le fer, du cuivre, du plomb, de l'antimoine, &c., qui sont précipités facilement par l'hydrogène sulfuré, même de solutions acides.† Ceci ne prouve donc pas que j'ai eu tort d'affirmer que l'eau contient réellement de l'hydrogène sulfuré et un sel de protoxyde de fer ; mais s'il m'en fallait une nouvelle preuve, j'ai pour moi l'autorité de M. de Rottermund lui-même. Il dit, p. 10 de son rapport déjà cité, que les eaux de cette source contiennent les gaz hydrosulfurique et carbonique, du sulfate de protoxyde de fer, du sulfate d'alumine, de la potasse, &c. ; mais maintenant qu'il convient à son but de prouver que l'hydrogène sulfuré ne peut pas exister avec un sel de protoxyde de fer, il oublie ses premiers exposés. "Soyez-donc compatible" avec vos assertions, M. de Rottermund.

Il parle ensuite de confusion dans le procédé suivi pour la détermination du fer, et montre, pour en dire le moins, une ignorance inexcusable du langage ordinaire ou des procédés de la chimie. Je ne dis pas que j'ai trouvé dans la source, soit le protoxyde, soit le peroxyde de fer, mais que *le fer y existe comme proto-sel, que je le déterminai comme peroxyde*, d'après la méthode ordinaire,‡ et *le calculai ensuite comme proto-sulfate, ou sulfate de protoxyde de fer*.

Je n'ignore pas les obstacles offerts par des matières organiques à la précipitation de certaines substances minérales ; mais la chimie moderne a des moyens bien simples pour surmonter cette difficulté.§ Plus bas, M. de R. parle de l'acide phosphorique, dont j'ai dit qu'il existe des traces dans cette source, et

* Rose, Traité Pratique, tom. i. p. 69.

† Id. Traité Pratique, tom. ii. p. 131, et Mémoires de Will et Walchner, déjà cités.

‡ Rose, Traité de Chimie, tom. ii. p. 61.

§ Id. Traité Pratique, tom. i. p. 72 ; aussi Fresenius, Bulletin de la Soc. Chim. de Londres, part. ix. p. 130.

dit: "Je suis fâché qu'il n'ait pas voulu donner la description par quel réactif, et dans quelle période de l'analyse il l'a remarqué." S'il avait lu tout l'article, il n'aurait pu manquer de voir qu'à la page 152, j'ai décrit pleinement le procédé suivi, qui est celui que recommande Fresenius pour la séparation de l'acide phosphorique et de l'alumine.*

Pour ce qui est de la "correction" faite dans les analyses de M. Croft, il avait donné dans sa détermination, l'alumine et le fer précipité ensemble comme peroxyde de fer, et j'ai, pour comparaison, additionné les quantités de ces deux ingrédients, telles que déterminées par moi. La prétendue confusion qui embarrasse et amuse en même temps la critique, n'existe que dans son intellect.

Mais voyons pour les chiffres: il demande "qu'est-ce que cela signifie que M. Hunt trouve dans la même source la quantité d'acide sulfurique avec la formule SO_3 et ensuite SHO_4 ?" Ne sait-il pas que SO_3 est l'acide anhydre, que, suivant la coutume, on calcule toujours en spécifiant les déterminations d'une analyse, tandis que l'excès d'acide en outre de ce qu'il faut pour former des sels avec les bases présentes, est représenté comme combiné avec un équivalent d'eau pour former le composé $\text{SHO}_4 = (\text{SO}_3, \text{HO})$, qui, dois-je le dire pour l'information de M. de Rottermund, est l'huile de vitriol, et non l'acide de Nordhausen. Comme c'est la seule combinaison stable de l'acide anhydre avec l'eau, il est présumable que c'est celle qui existe dans l'acide sulfurique dilué, et dans la source de Tuscarora. Si les recherches de M. de Rottermund ont jeté quelque nouveau jour sur les combinaisons de l'acide sulfurique avec l'eau, je serai charmé de l'apprendre.

En finissant, je prendrai la liberté de dire que mes observations n'ont pas été dictées par un sentiment de malveillance envers M. de Rottermund, mais bien par le désir de lui faire voir les nombreuses erreurs dans lesquelles il est tombé, pour n'être qu'imparfaitement au fait du sujet qu'il a traité, et je me flatte qu'il recevra comme venant d'un ami, le conseil que je lui donne de sauver sa réputation, sinon en qualité d'un "simple citoyen" qui joue le rôle de critique, du moins en celle d'un

* Fresenius Anal. Quant. p. 259.

chimiste "reconnu comme tel par les premiers chimistes de l'Académie des Sciences de Paris," en gardant le silence, à l'avenir.

Laboratoire de l'Exploration Géologique,
Montréal, 18 Déc. 1849.

We have to acknowledge having received a lengthy paper, being a rejoinder from E. S. de Rottermund Esq., to the reply of Mr. Hunt, Chemist of the Geological Survey. Howsoever much we desire to oblige M. de Rottermund, we feel compelled to decline his paper, inasmuch, as the grounds taken by that gentleman, differ but very slightly from those in his critique, and to which in common fairness to Mr. Hunt, we have opened already our columns for that gentleman's reply. As the question now descends into a personal controversy, uninteresting to a majority of our readers, Mr. de R. will excuse us for declining its introduction into our pages.—Editor of British American Journal, Feb. 1850, p. 279.

REPLY TO THE OBSERVATIONS OF M. DE ROTTERMUND,

ON THE CHEMICAL PORTION OF THE REPORT OF THE GEOLOGICAL
SURVEY FOR THE YEAR 1849.

*To the Editor of the Globe.**

MR. EDITOR,—I take the liberty of requesting a place in your journal for a few remarks on an article published by M. de Rottermund in *L'Avenir* of the 5th of October last. The article contains a violent attack on Mr. Hunt, Chemist to the Geological Survey, being a continuation of a series of papers directed against that gentleman's scientific labors, and which have appeared formerly in the Montreal *Medical Gazette*, and latterly in the above mentioned newspaper.

Some few years ago, I had occasion to expose the ignorance of M. de Rottermund, in some papers on the Tuscarora Sour Spring, published in the *Medical Gazette*, and I had hoped that I should never again have felt myself called upon to resume the subject, partly because, in such a discussion, one has to

* Copied from the Pilot Newspaper of Nov. 23rd. 1850.

descend to such trifling particulars regarding the most elementary principles of the science, and partly because, as the American Senator so justly observed, "kicking at nothing does wrench dreadful."

The unwarrantable attacks on the talented Chemist of the Survey, are however continued with such unwearied pertinacity, such utter indifference to scientific truths, such unmeaning verbiage, and inexplicable self-confidence, that I feel it to be a duty which I owe to the public, once more to expose the absurdities of M. de Rottermund. The peculiar style in which this gentleman indulges, is well adapted to impose on many who may not have any intimate acquaintance with the science of Chemistry. They may easily and naturally be led to imagine that he has entirely annihilated Mr. Hunt; they may think that the discussion is between two scientific men of nearly equal attainments, whereas we have on the one hand as accomplished a Chemist, (I do not hesitate to say it, notwithstanding M. de Rottermund's sneers) as any in America, —and on the other, a gentleman, who, from his ignorance of the commonest principles of the science, and his inability to comprehend the plainest language and the most straight-forward reasoning, can scarcely be considered as anything better than a charlatan.

Mr. Hunt has very properly refrained from answering these latter attacks, as being entirely beneath his notice; but his friends cannot afford to be so forbearing, especially as they are aware of the real cause of all this malevolence. M. de Rottermund was Chemist to the Geological Survey, and fully proved his incompetency, and Mr. Hunt occupies his place. *Hinc illæ lachrymæ!*

M. de Rottermund (who now appends to his signature the titles of "*Ancien Professeur de Chimie Analytique à l'Ecole Normale de Bruxelles; et membre correspondant de la Société Polytechnique de Paris,*") has got the funniest way of criticising that can well be imagined,—one would almost think he is in joke the whole time. For example, he commences his paper by an extract from Mr. Hunt's report, in which that gentleman attempts to account for the low temperature of a spring, by the escape of a large quantity of gas from it; and at this the

"Ancien Professeur" is exceedingly facetious, saying, "*D'après cette théorie, on pourrait faire geler du vin de champagne en ôtant le bouchon de la bouteille pour faciliter le gaz de sortir, et on aurait le vin de champagne frappé à la glace, à bon marché.*"

The veriest tyro knows that the escape of gas from a liquid produces cold—that liquid carbonic acid and other gases produce intense cold during their passage from the fluid to the æriform state, and a thousand similar instances, so that all the learned *Professeur de chimie analytique* effects by his witticism, is to show his lamentable ignorance of the laws of latent heat.

In the next paragraph M. de Rottermund refers to his criticism, (*L'Avenir*, 29th January, 1850,) on the labors of Mr. Hunt for the preceding year. That article is far too long and too stupid to enter into at present. I will merely remark with regard to it, that having with most incredible perseverance accomplished the perusal of seven closely printed columns of the most abominable trash I ever read, I can safely say that there is not one point touched upon, in which Mr. Hunt is not perfectly right, and M. de Rottermund most egregiously wrong.

In criticising Mr. Hunt's analysis of the water of Varennes, M. de Rottermund, as a matter of course, exposes himself most delectably. Mr. Hunt having found carbonate of strontia and of baryta in the spring, describes the process by which he separated them, and ascertained their quantities. The course adopted by him is exactly that which every Chemist who understands his business would pursue; in fact, the perfect separation of the two substances could not be effected any how else, and there is not one part of the "filtering and heating, heating and filtering," to which M. de Rottermund so strongly objects, that could by any possibility be omitted. M. de R. has probably reasoned thus:—Here are carbonates of baryta and strontia; there is no use in bothering myself with the tedious and useless process by which such poor, ignorant creatures as Berzelius and Dumas, Rose and Berthier, have effected their analyses; let us sprinkle a pinch of salt on the tails of each of the carbonates, and there we have them.

Deeply as we must regret the loss which the students of the "*Ecole Normale de Bruxelles*" must have sustained in being deprived of the invaluable prelections of the learned "*Professeur*

de chimie analytique," we in Canada may flatter ourselves with the hope that he will soon make known to the world those brilliant discoveries by which he dispenses with all those tedious operations which we, benighted individuals, have been taught to consider essential.

In another paragraph, M. de Rottermund finds fault with Mr. Hunt's process for converting sulphate into carbonate of baryta, by fusion with carbonate of soda, and supports his argument by a quotation from Berzelius. He very ingeniously stops his quotation with an &c., which unfortunately contains the most important particulars, viz: that the substances are mixed in their atomic proportions! Is the learned professor of analytical chemistry so utterly ignorant of the ordinary processes of analysis, as not to know that an excess of the flux is always employed in such cases? "*Que le public juge donc de la valeur de tous les travaux qui sont présentés dans ce rapport.*" Rather let the public estimate at their true value the ridiculous pretensions of this would-be critic.

In another paragraph he finds fault with Mr. Hunt for not including sulphuretted hydrogen in the list of substances found in a *sulphurous* spring. Is the learned professor unaware that the term sulphurous is only applied to such springs as contain this gas, but that the nature of the salts may vary very considerably? Mr. Hunt omitted the mention of sulphuretted hydrogen, because he had previously employed the term *sulphurous*, which one would imagine ought to be sufficient for any person not endowed with the peculiar obtuseness of M. de Rottermund.

The next attack is upon an analysis of a mineral from the Poisson-Blanc, and here, as might be expected, M. de Rottermund exhibits as much ignorance of mineralogy as he had previously shown with regard to chemistry.

Mr. Hunt gives the quantities of the different substances which he found in the specimen, and M. de R. asks whether this is a scientific analysis. In the name of all that is incomprehensible, what does he want? Mr. Hunt states the nature of the ingredients, and even classifies them in his arrangement, showing which belong to the metallic and which to the silicious or earthy portions of the mineral. It is perfectly evident, on

first inspection of the analysis, that the mineral is a mixture of one or more metallic sulphurets or sulpho-arseniurets, with a silicate and carbonate, and Mr. Hunt mentions this fact. M. de Rottermund, however, with the greatest ingenuity, runs his head against the hardness, crystalline form, specific gravity, and atomic constitution of a body which is plainly a mixture, a circumstance which renders the determination of the above properties either utterly impossible, or if possible entirely worthless, in a mineralogical point of view. The mineral was analysed for the purpose of ascertaining how much nickel and cobalt it contained, and Mr. Hunt tells us that he found in the rough specimens 8.26 cent of nickel with a trace of cobalt, and that if the earthy and silicious portion were removed, the metallic part would contain 13.93 per cent of the same ingredients. (I may mention, in corroboration of this, that I have myself analysed a similar mixture from the Wallace mine, sent to me under the name of gray ore, and found 13.00 per cent of nickel with a trace of cobalt.)

The industrial value of the mineral has, therefore, been clearly shown by this analysis, and its nature demonstrated, the long rhodomontade of M. de Rottermund being as devoid of value as it is of meaning. The only discovery that the learned ex-professor seems to have made is a mathematical one, viz: that the proportion $59.30 : 8.26 :: 100 : X$, is equivalent to

$$X = \frac{8.26 \times 100}{59.30}$$

which will be a great delight to all mathematicians.

So much for M. de Rottermund's criticism of Mr. Hunt's researches; in his attempts to injure Mr. Hunt's reputation as a chemist, we scarcely know which to admire most, the matchless ignorance of the writer, or the almost incredible reliance that he places on the supposed incapacity of the people of Canada, to distinguish between paste and diamond, glittering tinsel and genuine metal.

Let M. de Rottermund come forward with some practical experiment to prove that he knows what he is talking about; let him describe his trials, their results, and his deductions, and then Mr. Hunt or I will be willing to enter the arena with

him in scientific discussion ; up to the present time he has not given us the details of a single analysis, quantitative, or qualitative of any ore, mineral or mineral water, and until he does so, I cannot but join Mr. Hunt in advising him to remain silent, even although to the recommendation of being "*reconnu par les premiers chimistes de l'Académie de Sciences de Paris*," he now adds the title of "*Ancien Professeur de la Chimie Analytique à l'Ecole Normale de Bruxelles*," &c. &c. &c.

I remain, &c.,

HENRY CROFT, Toronto.